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MSS. intended for publication and books, etc., intended for review should be sent to Professor J. McKeen Cattell, Garrison-on-Hudson, N. Y.

BIOLOGICAL RESEARCH; THE VALUE AND THE DANGER[1]

I SEIZE with avidity the opportunity furnished me as your retiring president to discuss a subject which I am anxious should have your earnest consideration. I am aware that the title which I have announced is ambiguous, but I think that before I am through there will be no lack of understanding as to what I have in mind. *Biological research* includes many things. I mean to limit myself to those phases of it which require the experimental use of living animals. And I grasp this opportunity because I feel that our colleagues in other fields of scientific effort do not always fully perceive the value of the knowledge attained by this means; and that they do not at all appreciate the danger that freedom of research in these lines may be seriously hampered by hostile legislation or misdirected public opinion.

You who are engaged in very various lines of research will appreciate the fact that the immediate application of a discovery is no fair measure of its value; the ultimate results of the knowledge obtained may become extremely far-reaching. It is in the creation of a background of knowledge and experience that the greatest good is to be obtained. Often we point to certain noteworthy achievements and at the same time fail to perceive that they have become possible only through the accumulation of a multitude of small details, the results attained by the humdrum plodding of patient mediocrity.

The creative imagination of genius is

[1] President's address before the California Chapter of the Sigma Xi, April 28, 1915.

creative, after all, only in the ability to make novel combinations of known elements. The successive steps in the progress of knowledge are absolutely essential. The difference between the genius and the common man is not that the former proceeds by longer steps, but that he takes them more rapidly—often so rapidly that he is hardly himself aware of the intermediate positions.

As an example of the results of the experimental method I want to speak first of the progress of knowledge of the circulatory system—the heart and blood vessels and their mode of functioning.

The beginning of definite scientific knowledge on this subject may be said to date from the publication by William Harvey in 1628, of "De Motu Cordis et Sanguinis," "The Movement of the Heart and Blood."

There was not lacking before the time of Harvey quite complete and accurate knowledge of anatomy of the organs of the circulation; the structure of the heart, the arrangement and distribution of the blood vessels, and the valves of the veins were well known. Notwithstanding this there existed in the minds of anatomists and medical men the most bizarre and remarkable explanations of the uses of these structures. I can perhaps illustrate no better than by a few quotations from Harvey showing the kind of notions against which he had to contend in teaching the doctrine of the circulation of the blood. In the Introduction to "De Motu Cordis et Sanguinis":

> Did the arteries in their diastole take air into their cavities as commonly stated and in their systole emit fuliginous vapors by the same pores of the flesh and skin; and further did they in the time intermediate between the diastole and the systole, contain air, and at all times either air, or spirits or fuliginous vapors, what should then be said to Galen, who wrote a book on purpose to show that the arteries contained blood only? . . .
>
> And if the arteries in their systole expel *fuliginous vapors* from their cavities through the pores of the flesh and skin, why not the *spirits*, which are said to be contained in these vessels, at the same time, since spirits are much more subtle than fuliginous vapors, or smoke?

But Harvey, instead of merely *speculating* upon the functions as they might be inferred from appearances in the dead animal, put everything possible to the test of *observation and experiment in the living animal,* and as a result was able to state his reasons for the belief in the circulation of the blood in language which can hardly be improved upon to-day. The different attitude of mind resulting from his practise of observation and experiment is shown in his assertion

> That the facts cognizable by the senses wait upon no opinions, and that the works of nature bow to no antiquity; for indeed there is nothing more ancient or of higher authority than nature.

Contrast with this the views against which he had to strive as shown by another quotation from the same book:

> Medical schools admit three kinds of spirits; the natural spirits flowing through the veins, the vital spirits through the arteries, and the animal spirits through the nerves. . . .
>
> Farther, besides the three orders of influxive spirits adverted to, a like number of inplanted or stationary spirits seem to be acknowledged; but we have found none of these spirits by dissection, neither in the veins, nerves, arteries, nor other parts of living animals.

It was never permitted Harvey to know the exact method by which the blood passed from the terminations of the arteries to the beginnings of the veins; for no microscope suitable for the observation of the capillaries had then been invented. This final step was reached by Malpighi in 1661 just four years after Harvey's death.

After the *fact* of the circulation had been established, it began to be possible to investigate the mode of working of the circulatory apparatus. The first important step in this direction was taken by the Reverend

Stephen Hales, a Church of England clergyman, who tied into the femoral artery of the horse a glass tube nine feet high and noted the height to which the blood rose. He was able to report an average pressure of the blood in the artery sufficient to support a column of liquid eight feet three inches in height, while the blood rose at the same time to less than one foot in the corresponding vein. He observed also fluctuations in pressure due to the individual heart beats, to the movements of respiration and to other causes. The details of his experiments were communicated to the Royal Society of which he was a fellow and were published in 1733 in a work entitled "Statical Essays, Containing Haemostaticks."

The method employed by Hales was extremely inconvenient on account of height of the tube. Moreover, it introduced a greatly disturbing factor, namely, the loss of blood from the vessels of the animal into the tube. These inconveniences were overcome by the use of the mercury manometer by Poiseuille (in 1828). But the careful and detailed study of blood pressure dates from the invention by Ludwig (1847) of an exact method of recording blood pressures. From that time onward, not only in Ludwig's laboratory where many of the generation of physiologists just passing were trained in the methods of their science, but in all the physiological laboratories of the world has the study of blood pressure been continued.

It is impossible here to summarize all the facts of importance that have been the outgrowth of these investigations, and of others, connected with the functions of the circulatory system, and which could have been learned in no other way than by experiments on living animals.

The heart is a pump driving an incompressible liquid through a completely closed system of branching elastic tubes, the terminal connections of the outflow and inflow portions of the system being all of capillary size. The study of this system presents a series of difficult problems in *hydrodynamics,* in which all the relations of force, rate and output of the pump, the heart, and the pressure, and friction conditions in the arteries, veins and capillaries must be considered.

But this machinery is all composed of living tissues which are interacting and self regulatory to an extraordinary degree.

The discovery by Claude Bernard, and others, of the existence of vasomotor nerves through which the caliber of the arteries may be changed, regulated and controlled, thus adjusting the resistance to the ability of the heart, and also providing that the heavier flow of blood may be shunted from one set of organs to another according to the needs of the body, is of prime importance; so also was the discovery by Weber of the inhibitory action of the vagus nerve upon the heart, which, acting like a brake on that organ, keeps its action always under definite control; and the discovery by v. Cyon of the accelerator nerves whose function is in direct opposition to that of the vagus. Further, v. Cyon found that a special nerve, the depressor, carrying impulses from the heart and the great blood vessels to the brain, causes, when excited, a dilatation of the peripheral vessels and consequent reduction of the pressure against which the heart must work. None of these things could have been guessed from the study of the anatomical structures, nor could ever have been found out in any other possible way than by experiments on living animals.

But it may be asked, Has this knowledge any value? Has it any practical application? Is it useful only for the gratification of mere curiosity?

Putting aside for the present the implication in the expression "mere curiosity" which we hear so often in this connection I may answer that it is now possible to measure the blood pressure in man without resort to the method of Hales; no blood vessel has to be opened and no pain has to be inflicted. Blood pressure determination forms a part of every examination for life insurance, and of the routine of nearly every present-day medical examination. In certain conditions its measurement is of the most extreme importance. It gives exact, quantitative information on the state of the heart and blood vessels that could be obtained in no other way. And the usefulness of this information so far from being confined to diagnosis of disease of these organs themselves is quite as important in the light it throws on the functioning of other organs.

I have given this rather disproportionately long statement of the physiology of the circulation to make very specific what I mean in saying that the importance of most investigations is to be found not in the direct application of the specific discoveries but in the reflex effect of these on all related work. Antivivisectionists use the knowledge which has been obtained by experiments on living animals. No modern physician can for a single hour free himself from the deepest obligation to vivisection experiments, although he may never himself have made such experiments.

It is quite true that human blood pressure may now be determined without opening an artery and that the principles may now be explained without appeal to animal experiments; but I believe it to be equally true that this would not now be possible, and that neither the method of blood pressure determination nor its significance would now be known if the long series of vivisections had not first occurred.

In this connection it will be appropriate to say a word about surgical shock. Every one realizes that as surgery is practised today the chance of coming out of a major surgical operation is always good, yet it is no light matter; there is usually real danger; and the memory still remains with us of friends or acquaintances who in an otherwise not serious operation succumbed to shock. Shock is a peculiar complex not easy to define. There is not usually the suddenness which the word implies to the lay mind; but there is a great depression of the functional activities; and most marked of all its symptoms is an excessive fall of blood pressure. To discover the real nature of shock and thus to furnish the surgeon the means of its avoidance is no small boon to humanity. With this purpose in view many researches have been carried on. It has not been easy to find the true cause of the lowered blood pressure but much progress has been made. Perhaps no one man has made such untiring efforts to the solution of this problem as George W. Crile, and for this he has been denominated "brute," "savage," "arch fiend," "torturer" and almost every other term which fanaticism can devise. And yet it is to Crile and the system of *anoci-association* which he has worked out that every man or woman who has to undergo a major surgical operation owes a debt of gratitude which he can never repay; for not only does this method, where applicable, reduce the immediate danger from surgical shock, but it also greatly reduces or wholly sets aside the long period of nervous impairment which so commonly follows recovery from an operation in which these principles are disregarded.

I have selected the history of the study of blood pressure on account of its comparative freedom from those details which appeal to the emotional and dramatic side of human nature. I have used it to illus-

trate the growth of knowledge sought for its own sake into knowledge which is applied to the good of man. But I have merely touched upon the latter. Let me emphasize again that the great importance is not in the value of this or that specific detail, but in the great background which has been built up, which enables us to gain and to interpret new knowledge, and to see things in a proper perspective.

The physiology of digestion might have served equally well to illustrate the same truths. We owe to Pawlow and other workers in this line a mass of knowledge of prime importance to man, and this could not have been obtained in any other way than by vivisection. It is true that a few unfortunate human beings have had gastric fistulas formed through accident, and they have been used to study processes going on in the living stomach. But these studies have had no such orderliness as those in which upon animals definitely planned and controllable operations have been made. Indeed, the human observations have been mainly useful to check up the observations on animals and to see whether for some reason conclusions drawn from animals might not be wholly applicable to man.

To experiments on living animals we owe most of what is known of the functions of the various parts of the nervous system. The possibility of diagnosis of the seat of nerve tumors, of injuries, of pressure due to blood-clot and the like, in many instances depends upon knowledge of cerebral localization first discovered by experiment on the brain of the dog.

We are just now at the entrance into a new era in the history of physiological science. The study of the glands of internal secretion is widening and deepening our vision of the life processes, and I confidently believe that the next decade or two will be most fruitful in this comparatively new field of research. Already we have use of adrenalin, and various gland extracts. Nearly all our knowledge is based on vivisection.

It is not the purpose of this paper to go into the enumeration of specific instances of the *value* of biological research; many of them are already familiar; some of them are, rightly considered, among the greatest achievements of the human race. You know that the event celebrated by the great Exposition whose lights are at this moment blazing across the Bay could not have been accomplished if malaria had not first been conquered through biological research; you know that Havana by the same means has been changed from a seed bed of yellow fever to a healthy port and has ceased to be a menace to our own southern coast. You know that while occasional deaths from diphtheria still occur, the intelligent use of antitoxin has dispelled the dread and the terror which its presence in any community formerly produced; that a knowledge of the Pasteur treatment for rabies has reduced the death rate from that horrible disease from 15 per cent. to about 0.3 of 1 per cent. These and the like achievements are what the Hon. Stephen Coleridge, president of the English Antivivisection organization has eloquently denominated "*The desolating advance of science.*"

You know all these things and much more, of the *value* of the achievements of biological research. But you probably do not know or, knowing, do not realize the vigor of opposition to all this effort for the advance of knowledge and the good of humanity. The *danger* of limiting, harmful restrictions is imminent and absolute prohibition is not improbable.

In England for years it has been necessary, if one wishes to perform a single vivisection experiment, to procure a license. It

is much easier to get a license to run a low grogshop. Any man may without a license and with practically no regard for the sensations of the animals rip out the testes from a boar or a dog, merely because it suits his convenience or his whim or his purse to have his animals gelded; but if a physiologist wishes to make the same operation for the purpose of scientific observation on the effects of castration he must secure a license stating with precision the building where this is to be done, and the purpose of the experiment, and he MUST, he has no option, perform the operation under complete anesthesia.

In this country at present the opponents of biological research point to England as the model country. But in England they continue the agitation for further regulation or complete prohibition, and they continue to persecute the licensees with persistent vilification and misrepresentation.

It would be out of place for me to take your time in a statement of the peculiarly extravagant and unscientific views of the opponents of biological and medical research if it were not that there is a real danger of the enactment of pernicious and obstructive legislation. A situation exists in which we who are doing what we believe to be an important work for humanity need your active cooperation, sympathy and support.

1. Practically all antivisectionists agree in the charge that experiments on living animals are necessarily cruel.

Now cruelty implies the infliction of needless or avoidable pain. No one justifies or can justify cruelty in experimentation any more than he can justify cruelty in any other action. But in the question of pain the unbiased individual will see that no one is so well qualified to judge as the experienced physiologist or surgeon. It would require the whole evening to discuss this one subject. Allow me to point out in brief the following:

The experimenter, even if he were really cruel, would usually defeat his own ends by the infliction of pain (*a*) because the pain impulses would cause disturbance of the normal functions which he seeks to discover and (*b*) because the struggles of a suffering animal would disturb the adjustment of apparatus and prevent the desired observation. It is the total ignorance of the real situation that causes so much emphasis to be laid upon this point by the opponents of research.

On the other hand, it is the fact that most vivisection experiments as actually performed, are done under deep anesthesia or narcosis—usually for obvious reasons much deeper than would dare be employed in human surgery. Now the opponents of research insist that anesthetics are not given, or that when given the attempt at anesthesia is a mere blind, and that the animals are allowed to undergo torture. Most of this discussion is by people who never gave an anesthetic, who would not know when an animal could be judged unconscious, and who would be unable to form an intelligent opinion as to whether movements of the animal were unconscious reflexes or purposeful struggles.

But why assume, as every one of the antivivisectionists does seem to assume, that all persons engaged in animal experimentation are necessarily cruel? As one reads their publications he finds that always the experimenter is supposed to delight in torture. In fact he is spoken of over and over again as "arch-fiend," "torturer," "devil in human form" and the like. Can they see no other purpose? No other motive? Has the eminent head of our department of pathology exposed himself week after week to the danger of infection with typhoid, tuberculosis and what not, merely because

he has a fiendish delight in seeing the quivering of flesh and hearing the plaintive squeal of guinea-pigs when he thrusts the hypodermic into them? Why would not a plain needle serve equally? The point of view is so absurd that it should require no discussion among intelligent people.

It is charged, however, that the practise of vivisection tends to induce a disregard for the sufferings of animals and brutalizes the mind and conduct of the experimenter. Now it happens that I have a pretty wide acquaintance among physiologists, and I have known some of the most accused vivisectors rather intimately. Of course they are not all alike, they differ as other men differ. But on the average in point of humane, kindly sympathy they stand above their colleagues. And the reason for this is clear to him who will listen to reason. They have gone into this work because the higher human sympathy has appealed to them; they have sought earnestly for those things which will relieve or prevent suffering; their lives are given to the solution of problems the ultimate end of which is found in the very things about which they are charged to be wholly indifferent.

Not only is it not true that vivisection experiments tend to make the experimenter callous; the reverse is actually the case. I can testify from my own experience that it is harder to make the fiftieth experiment than the first; that one's sympathies are more and more awakened rather than destroyed. There is no doubt that abuses are possible—are even probable. Yet most of the stories told to illustrate the brutality of vivisectors in things aside from the experiments themselves are highly improbable. As, for example, the statement that Dr. Sweet, of the University of Pennsylvania, kicked across the basement floor a poor emaciated dog on which he had operated. An operated animal is too valuable to be used in this way.

Were I to descend to the methods of our detractors I might use the following from my own experience to prove that antivivisection doctrine induces brutality.

I was once teaching in a small college the president of which was an ardent antivivisectionist. One day I received by mail a large poisonous centipede, carelessly enclosed in an unlabeled box. I made haste to get it into a wide-mouthed bottle. I had just succeeded when the president came into the room followed by a stray dog. There was a rule that dogs were not to be allowed in these rooms. The president took me roughly to task for allowing the poor centipede to suffer for lack of air in the bottle. Then, seeing the dog, he asked if it was mine? When I told him it was not he ordered it out of the room. The poor animal instead of obeying crouched on the floor and the president kicked it brutally and cruelly across the room and through the entance. Yet he could declaim with tearful voice upon Llewellyn's faithful hound Gelert!

It should be emphasized here that the lower animals themselves gain immensely from the results of vivisection and of experiments on living animals. The same advantages of protective serums and antitoxins are made available for them as for the human. The Agriculture Department of the University of California at the present time makes and distributes hog cholera serum. The Report of the College of Agriculture for the year 1913–14 states that when a herd of hogs becomes infected and is not treated with serum forty to eighty per cent. of the animals die. I am told by experienced and unprejudiced stock raisers that this estimate errs on the side of conservatism. The report shows further the following figures for a diseased herd treated with the serum:

Died before vaccination	92
Sick when vaccinated	123

Number vaccinated	1,656
Died after vaccination	233
Per cent. of loss	19

That is the conservative statement of the report. There should actually be deducted the 123 sick when vaccinated, for vaccination does not help those aleady sick with the disease. That reduces the percentage to 15 as compared with 40 to 80 per cent. when unvaccinated.

Now the vaccine can not be prepared without operation on living animals; and the method and the underlying idea could never have been reached except through animal experimentation.

This may serve as a near-by example of what is done and as a forecast of what will be done for the animals themselves. The very beginning of Pasteur's famous work was for the conservation of animal health. *To this really great end none of the opponents of vivisection has contributed an iota.*

2. It is affirmed by most antivivisectionists that experiments on animals are useless in that no knowledge of any real value has ever been attained in that way. This attitude is well illustrated in a recent circular entitled "Claim Everything" issued by the American Antivivisection Society. This circular is intended to be a rebuttal to an article by Dr. W. W. Keen in the *Scientific American* of June 20, 1914. The statements in the circular are on the authority of the president of the British Union for the Abolition of Vivisection. The circular says,

> Brain surgery owes nothing to animal experimentation. In brain, above all, the animal differs from man.

This appeals to a multitude of voters who do not know that motor localization was discovered by Fritsch and Hitzig on the brain of a dog. Dr. Keen had referred to the new and highly successful methods of direct transfusion of blood. The circular states,

> The direct transfusion of blood needs no experiments with animals, nor is the operation itself necessary.

The curious psychological twist in the reasoning of the opponents of progress in scientific medicine is shown in the following quotations from the same circular, copied verbatim, except that to save the space of comment I have inserted the italics:

> Operations for goiter, again, depend upon the *aseptic treatment.*
>
> Diphtheria has been reduced solely by *sanitary measures.*
>
> Malaria has been abolished by *sanitation.*
>
> Yellow fever can not have been abolished by any means based on experiments on animals, *because the germ has never been found* to experiment with.
>
> Discovery of salvarsan. This had better never have been made.

Every one familiar with the history of hygiene and sanitation knows how much of our knowledge and our point of view has been obtained through experiments on animals. Prohibit animal experimentation and progress in hygiene and sanitation would be practically brought to a standstill. Yet the opponents of research reiterate the statement that hygiene and not experimentation has enabled us to advance, and hence that experimentation is useless. Where a forward step has been made which is not attributable to "hygiene," as in the case of direct blood transfusion, its usefulness is flatly denied.

In most literature of this kind you will find expressed or implied a denial of the whole range of scientific knowledge as to the relation of microbes to disease. They refer to serums, vaccines and antitoxins in terms of profound contempt. A favorite expression is one which I have heard used by a California legislator, who calls vaccine "rotten animal pus" and who would make it a criminal offense to introduce any vaccine into the human system.

3. *Another argument is based on the so-called "rights of animals."* As a question of theoretical ethics I am willing to leave that for the present to the philosophers. I can not argue with the man who insists that his dog and his hog are as good as he is; that he has no right to restrain the one or to eat the other. If he refuse to eat meat, or eggs, drink milk, use leather, wool or other animal products for clothing or shelter; if he refuse to make counter attacks against the lions or serpents which attack him, he is consistent; I can not argue with him; I can merely watch him go his way in the procession with the trilobite, the ichthyosaurus and the dodo. But intensely practical questions arise and must be met. And the life of a relatively few animals is placed against the life and health and comfort of the human race. The antivivisectionist insists that even if you grant that the injury to the guinea-pig or the rabbit or the horse will save the life of a child you have no right to save it in that way. If there is not room in the life-boat for the woman and the dog you have no right to push out the dog to make room for the woman.

But here I want to take issue squarely with the claim that we have no right to make experiments which cause pain—that is a fatal admission which some of the English physiologists have made. We have a right to perform painful experiments if the knowledge that we seek can be obtained in no other way. Ordinarily it can be obtained better without pain, or can only be obtained in the absence of pain, but the principle remains. So long as man lives in the same world with other animals, eating to some extent the same food, subject to a large extent to the same diseases, it will be necessary for man either to maintain the mastery or to become one of the beasts of the field himself.

But especially I can not see why experiments for the good of humanity and for the benefit of the animals themselves should be prohibited on the ground of cruelty and the absence of right, in the light of the permission of many other things. The castration of an animal as performed on the farm by far exceeds in cruelty and callousness of performance anything which I have ever witnessed in a laboratory. A few hundred animals are used in all our laboratories for all purposes. The census report shows that in California in 1909, there were born 163,728 bull calves. It is fair to assume that 150,000 were castrated. There were born 41,927 colts. Of these approximately one half were probably males, and making deductions for those kept as stallions, there were here at the lowest estimate 19,000 geldings. There were 283,741 pigs born, which means probably 135,000 males to have their testes ripped out. A total each year in California of 304,000 operations. Comparing these in number and violence with the work in biological laboratories and medical schools, the latter becomes wholly insignificant. But the gelding of the boar does not have the emotional appeal in it and we hear little about it. Dehorning of cattle is a painful operation, but it saves vastly more pain which would result from the injury which, without it, they would inflict upon one another.

4. It is urged that certain results of undoubted value (or from the standpoint of the opponents of research, of possible value) could have been reached by some other way. This is a line of reasoning which has been used with a great flourish of apparent candor and show of plausibility. A biologist having by a long and painstaking series of experiments found the solution of a problem, a pettifogger takes that solution and shows by a play on words, how he could, without experiment, have de-

rived the same conclusion from certain given data. This is the basis of the constant appeal to hygiene, as the means of prevention of disease; while the very principles of hygiene are based throughout on animal experimentation.

I have read recently with great interest two books, accounts of journeys over practically the same ground, the journey from Mombasa on the East African coast to the great lakes at the source of the Nile. The one is by Lieutenant Speke, the other by Colonel Roosevelt. Speke traveled in constant danger and discomfort, beset with discouragements and the opposition of treacherous natives, in an unexplored, unknown land. Roosevelt made the trip by railroad. Our antivivisection opponents continually upbraid us for traveling like Speke in the difficult, uncharted territory, when we might wait and go *de luxe* in a Pullman: Will *they* build the road while *we* wait?

5. A further charge is urged against vivisection, that it leads to a state of mind which will not hesitate to make similar experiments on man. Human vivisection is held up as the acme of the fiendish impulses of the biologist, physiologist or surgeon. A hospital is a place of unspeakable horrors.

Now on this I must make two remarks. (1) That any thinking man will see that certain observations may be made on a patient without injury or pain to the patient, and that if these observations or experiments furnish useful knowledge, there can be no possible objection to them, and (2) every surgical operation is a vivisection experiment in one sense. A surgical friend has vivisected me, and yet I do not call him a fiend and an arch torturer.

Of course there are all sorts of men among physicians and surgeons as in all other professions. Abuses and outrages do occur, no doubt. There have been wicked doctors who have abused their trust; and there have been clergymen with whom the virtue of a young lady boarder was not safe; but we need not say for this reason that all surgeons are arch torturers and that all preachers are arch-lechers.

And this brings me to a point I wish to insist upon, that just as you do not need tc pass a special law against adultery by ministers of the gospel, but that if you did so you would put an imputation on the character of a large body of earnest, sincere and unselfish men, so you should not pass laws which would put on men in biological research the imputation of bad faith and cruelty. Make the general laws against cruelty to animals as strict and far-reaching as may seem necessary for the good of the human race; but do not single out the men who are devoting their lives to the search after that knowledge which is for the best good of the race, and make them the special objects of unnecessary, restrictive limitations. If experiments on animals must be prohibited let the same law prohibit castration of animals and the dehorning of cattle. If the English law requiring all operations by a scientific man to be done under anesthesia be adopted, then require that the operations on the farm be performed in the same way.

You will perhaps say that the arguments mentioned are unworthy of attention; that it is beneath our dignity to answer them. It will not do to take that attitude. The opponents of research are too strong and too well organized to be neglected. They have enormous sums of money at their disposal. They have been able to subsidize newspapers and are prepared for a campaign of persecution and prosecution. The opponents of research are not easy to classify. They represent widely varied types of mind, but the following are usually recognizable:

1. The fanatics. This type is represented by the man who states over his signature that he would prefer to have his own child die of diphtheria than to have it saved by the torture (?) of a single guinea-pig. These are perhaps the only thoroughly consistent antivivisectionists. They are often so much in earnest that they do not hesitate to mislead the public through publication of untruths.

2. The cultured ignoramuses. A large class of people highly educated along certain lines of language and literature, but profoundly ignorant of the most simple and fundamental facts of natural law. They are the Clara Vere de Veres of both sexes and all ages.

3. The financially interested. Great fortunes are accumulated by the sale of patent nostrums. The business makes headway in proportion as medical knowledge and medical practise can be thrown into disrepute. Thus the *Journal of Zoophily*, January, 1915, quotes the following with no word of disapproval.

Medical Freedom says in its October number:

"Only recently Mrs. Catherine E. Mercer and her two children were vaccinated against typhoid in Brooklyn, N. Y. All were made ill. Mrs. Mercer died and the two children suffered for weeks. In Iowa a perfectly healthy guardsman was vaccinated against typhoid, became ill and died. In Camp Dodge, Des Moines, Iowa, Conrad Liljeberg died soon after vaccination. Also Clarence Pantzer, Thirteenth Coast Artillery, National Guard, New York."

4. Religious cults. It must be said to the credit of the majority of those who profess a religious philosophy which ignores disease that they are not inclined to put obstacles in the way of medical progress. Nevertheless, in a recent number of the *Journal of Zoophily,* a column headed Anti-Vivisection Notes is entirely occupied by a long tirade against the medical profession by the senior senator from California.

5. Demagogues. These are not wanting and in California have been not unsuccessful in securing legislative position by masquerading as benefactors and reformers.

The above are strange bedfellows, but they seem to agree well among themselves. They have this more or less in common that they desire to throw the efforts of the earnest, honest physician into disrepute; his loss is their gain. Anything does for a pretext. It can be vivisection or vaccination or quarantine or what not. Their method is always that of the pettifogger or the demagogue. They publish accounts of experiments done under anesthesia and of experiments done before the introduction of anesthetics as if they were all alike and now all in vogue. They describe vivisections done in the days when men were hanged and quartered as if they were the common practise of to-day. And in it all the appeal is to sentiment and prejudice, not to reason and common sense. By these methods they reach and may yet more effectively influence large numbers of honest and conscientious voters too busy to inform themselves as to the real issues, and unable to unravel the tangle of sophistry, sentiment and misrepresentation, with the result that there is great danger of hostile and harmful legislation.

In the face of all this opposition I feel justified in calling for support from you who are working in the various fields of science more or less remote to that of biology, not only because as co-workers in the effort to enlarge the sphere of human knowledge as men of open mind and enlightened sympathies your support may rightly be expected by those whose researches are primarily concerned in the discovery of those truths that are directly applicable to the diminution of pain and suffering and disease. But I would also place before you the importance to all of

you in your various fields of that which I have called the larger background of knowledge. It is only by this that we can see things in their true perspective. Our respective sciences and our special fields of research become of value only when their wider relations are apprehended. And may I without unduly magnifying mine office as a biologist call your attention to the fact that biology has contributed no unworthy share to the means of progress in the sister sciences. The contributions of biologists, especially the workers in physiological chemistry, to the general advance of chemical science does not require to be mentioned; nor do I need to refer to the usefulness to physical chemistry of the fertile ideas of Pfeffer and De Vries in the explanation of osmotic pressure. The physicists do not need to be told that by far the most sensitive galvanometer for the measurement of minute currents of short duration is the device of the physiologist Einthoven, designed primarily for use in the study of living organs. The engineers will recall that the method of recording progressive changes on a revolving drum is the application of the kymographion invented by Ludwig for the recording of blood pressures; but now employed in securing graphic records of a great variety of natural phenomena.

Or let me reverse the picture and remind you that the physiologist, the pathologist and the physician are laboring to apply the results of your researches in the explanation of the normal life processes, and to use them in the discovery of the causes of pain and suffering and disease, to the end that these causes may be overcome. Toward this result all lines of scientific effort are contributory.

Samuel S. Maxwell
University of California

THE ERRORS IN PRECISE LEVELING DUE TO IRREGULAR ATMOSPHERIC REFRACTIONS[1]

Very accurate determinations of elevations above some datum have been made possible by the great improvements in the wye or spirit level which have taken place during the last half century. In 1867 the International Geodetic Association defined precise leveling as that which has a probable accidental error of not more than 3 mm. per kilometer. The leveling run to establish the controlling or fundamental elevations in the interior of the countries, during the decades which followed, showed these limits to be too liberal. In 1912 the International Geodetic Association adopted a resolution calling for a still higher grade of leveling called "leveling of high precision." This is defined as leveling in which every line, set of lines or net is run twice in opposite directions on different dates, as far as possible, and whose errors, computed by prescribed formulas, do not exceed ± 1 mm. per kilometer for the probable accidental error and ± 0.2 mm. per kilometer for the probable systematic error.

This class of leveling is easily secured with the modern instruments and methods. In fact the greater portion of the leveling done with the older instruments and methods in the United States by the Coast and Geodetic Survey and by other organizations came within these limits.

The datum or plane of reference which has been adopted in this and in other countries is mean sea level, that is the surface of the oceans with the water assumed to be at rest and affected only by gravity. This surface may be determined with relation to fixed points on land by long series of tidal observations. The mean surface varies in height from day to day, month to month, and even from year to year. Whether there are secular changes is not definitely known. The disturbing influences are the sun and moon, prevailing winds and varying atmospheric pressures. The configuration of the shore may have some

[1] Read before the Washington Philosophical Society, March 13, 1915.

effect, but this would probably be very slight at points on the open coast and would no doubt be constant and not show in the series of tidal results.

The following table gives the yearly averages for the tidal stations at the Presidio, San Francisco, just inside the Golden Gate.

Year	Height, Ft.	Year	Height, Ft.	Year	Height, Ft.	Year	Height, Ft.
1898	8.30	1902	8.57	1906	8.58	1910	8.42
1899	8.44	1903	8.53	1907	8.66	1911	8.61
1900	8.50	1904	8.63	1908	8.43	1912	8.49
1901	8.46	1905	8.65	1909	8.53	1913	8.51

The mean sea level for 16 years (1898 to 1913) equals 8.519 feet on the staff. The staff was frequently referred to a substantial bench mark near the tidal station and corrections were applied to take account of any variation in the elevation of the zero of the staff referred to this bench mark.

The total range during the 16 years was 0.36 foot. The greatest difference from the mean is 0.22 foot, while the average difference is 0.075 foot. It is seen that the mean value for any three consecutive years does not differ from the mean for the sixteen years by more than .110 foot. There are 14 3-year groups with an average difference from the mean of 0.04 foot, about 1.3 centimeters.

Ordinarily when a tidal station is established solely for determining mean sea level from which to extend a line of precise levels a series of tidal observations extending through only three years is made. Judging from the San Francisco records, we may therefore expect an uncertainty of the plane of 0.04 foot or 0.013 meter.

Whether or not the mean sea levels at different parts of the same ocean and of different oceans lie in the same equipotential surface is a question which has not been solved in the United States. It is true that the several transcontinental lines of levels indicate that the Pacific is higher than the Atlantic and Gulf, but this may be due to accumulated errors in the thousands of miles of levels involved. The results of careful leveling across the Isthmus of Panama show that the mean sea levels of the Atlantic and Pacific are in the same equipotential surface within 17.8 centimeters. This difference may be largely due to the unavoidable errors in the leveling and the determination of the sea levels of the two oceans. The mean sea level on the Pacific was determined by observations through only one year and may be in error several tenths of a foot.

The leveling across Florida is not strong and its results are not conclusive. There are four lines between St. Augustine on the Atlantic and Cedar Keys on the Gulf, with a total range of 0.85 meter in the difference in elevation of the two places. At each of those points tidal observations are being made and within a year or two a new line of levels will be run between them. We hope to obtain then some definite data in regard to the relative elevations of the two bodies of water.

In the absence of conclusive information we have assumed in our level net adjustment that the surfaces of the three great bodies of water touching this country are in the same equipotential surface and the starting points of the various lines of levels are consequently given zero elevations.

There are two ways in which the errors of leveling show; one in the closing of circuits, and the other in the disagreement in the difference in elevation between each two consecutive bench marks as determined by the two runnings of the line between them. We shall not have time to consider at length the closing errors. The closing errors of the circuits of levels run entirely with the new instruments and methods[2] are seldom greater than 0.20 millimeter per kilometer. This is a clear indication that the accumulative errors in a long line are small.

The following table shows the principal facts in regard to the closing errors of the 84 circuits forming the net which was adjusted in 1912.

There are many sources of error in leveling, of a systematic nature which may be made to

[2] See the following C. and G. S. publications: Appendix No. 3, Report for 1903, and Special Publications Nos. 18 and 22.

Rate per Km. in Mm.	Rate per Mile in Thousandths of Foot	No. of Lines	No. Km.	No. Miles	Per Cent. of Whole
.00 to .10	0.00 to 0.52	56	17,251	10,719	40.1
.10 " .20	0.52 " 1.05	36	8,708	5,411	20.3
.20 " .30	1.05 " 1.57	30	9,732	6,047	22.6
.30 " .40	1.57 " 2.10	7	1,480	920	3.4
.40 " .50	2.10 " 2.62	14	1,488	925	3.5
.50 " .60	2.62 " 3.15	4	492	306	1.1
.60 " .70	3.15 " 3.67	5	595	370	1.4
.70 " .80	3.67 " 4.20	1	312	194	0.7
.80 " .90	4.20 " 4.72	1	150	93	0.3
.90 " 1.00	4.72 " 5.25	5	647	402	1.5
1.00 " 2.00	5.25 " 10.50	13	2,076	1,290	4.8
Over 2.00	Over 10.50	1	48	30	0.1
		173	42,979	26,707	99.8

act as accidental errors in a long line of levels. The methods followed in the Coast and Geodetic Survey have this in view. One of the most troublesome errors encountered in the past was due to the changes in the relation between the line of sight and the axis of the bubble caused by rapid and unequal temperature changes in the different parts of the instrument. The older instruments were made of metals having large coefficients of expansion, and the bubble was at a considerable distance from the center of the telescope tube. It was found that the error of a line was a function of its direction or azimuth. This error is probably eliminated in leveling run with the Coast and Geodetic Survey level which has been in use about fifteen years. It is made of nickel-iron with a coefficient of expansion of only .000004 and its bubble is set into the tube of the telescope near the line of sight.

We will not consider the sources of errors which are well known and which are largely eliminated by the methods employed, but will confine ourselves to some interesting errors apparently due to variations in the vertical atmospheric refraction on steep grades, and even these can only be touched upon. They are considered at some length in a recent publication of the Survey.[3]

All leveling by the Survey is run in both directions, forward and backward, the line is divided into sections approximately one kilometer in length, and the two runnings of a section are made in different days in order to vary the atmospheric conditions. Usually one running is made in the morning and the other in the afternoon. For a number of years the observers have kept records of the time of day of the runnings of the different sections and the weather conditions which obtained. Five lines of levels were selected for a study of the possible relation between the errors of leveling and the conditions of the weather, the time of the observations and the steepness of the grade. These lines are:

No.	Line	Distance, Kms.	Direction of Progress	Average Length of Section, Kms.
1	San Francisco, Cal. to Marmol, Nev.	497	Eastward	0.8
2	Beowawe to Marmol, Nev.	451	Westward	0.9
3	Brigham, Utah, to Beowawe, Nev.	486	do.	0.8
4	Butte to Devon, Mont.	461	Northward	0.8
5	Pocatello, Idaho, to Butte, Mont.	415	do.	1.1
	Total	2310		

Mean grade per section, steep slopes. 16.6 meters
Mean grade per section, low slopes... 3.4 meters
Mean grade per section, all sections... 6.4 meters

The total length of these lines is 2,310 kilometers. As it was impracticable to investigate the relations between the size and sign of the discrepancy between the results of the two runnings of the sections and the many different grades, the leveling was separated into only two classes: First, those having grades exceeding ten meters and, second, those with smaller grades. As the average length of a section is about one kilometer a ten-meter grade corresponds to a grade of approximately one per cent. The average grade for the first class is 16.6 meters, for the second 3.4 meters, and for all the sections 6.4 meters.

We shall first see whether there is any difference in the elevation between the two ends of a section by the two runnings where one running is in the morning and the other in the afternoon. The direction of the slope is not considered. In the following table *A*

[3] See Special Publication No. 22, of the C. and G. Survey.

stands for the morning and P for the afternoon running. The discrepancies are given in millimeters.

MORNING AND AFTERNOON RUNNINGS COMPARED, WEATHER CONDITIONS IGNORED

	Steep Grade	Low Grade
Number of sections	188	761
$P-A$, total, positive	+ 496.4	+ 1,645.1
Mean discrepancy	+ 2.64	+ 2.16
Number of sections	144	629
$P-A$, total, negative	— 340.4	— 1,277.8
Mean discrepancy	— 2.36	— 2.03
Number of sections	332	1,390
Mean discrepancy	2.52	2.10
Accumulated discrepancy	+ 156.0	+ 367.3
Mean accumulation per section	+ 0.47	+ 0.26

For both classes the sections with positive values of $P-A$ predominate while the mean accumulated discrepancy per section for the low grades is only 0.55 of that for the steep grades.

The next table includes the morning and afternoon runnings, which were made in sunshine.

MORNING AND AFTERNOON RUNNINGS, ALL IN SUNSHINE

	Steep Grade	Low Grade
Number of sections	131	529
$P-A$, total, positive	+ 358.6	+ 1,140.8
Mean discrepancy	+ 2.74	+ 2.15
Number of sections	87	456
$P-A$, total, negative	— 203.0	— 935.5
Mean discrepancy	— 2.33	— 2.05
Number of sections	218	985
Mean discrepancy	2.58	2.11
Accumulated discrepancy	+ 155.6	+ 205.3
Mean discrepancy per section	+ 0.71	+ 0.21

The evidence here is similar to that of the preceding table but the accumulated discrepancy for the steep grades is three and one half times as great as that for the lower grades.

The investigation does not indicate whether the morning or the afternoon running gives a value nearer the truth, but it is the speaker's opinion that the afternoon is freer from error than the morning running.

It is the speaker's opinion that the afternoon running gives on an average a difference which is closer to the truth than the morning running. In the afternoon the temperatures of the ground and of the air are more nearly the same and a layer of air of uniform density should be concentric or nearly so with the sea-level surface. If this is true the refraction on the front and back sights should be about the same. The leveling of the U. S. Coast and Geodetic Survey is seldom done after 5 o'clock in the afternoon. So the afternoon running is not materially affected by the abnormal refraction of the late afternoon when a line of sight on a grade would pass through layers of colder and denser air which would tend to be concentric with the surface of the ground. In the late afternoon the earth cools more rapidly than the air and the air near the earth's surface becomes colder than the air above and consequently denser than normal.

In the morning on a clear day the air is receiving radiated heat from the earth's surface. This decreases the density of the air close to the ground, and forms layers which tend to be concentric with the surface of the ground rather than with the sea level surface. (The air near the earth is of course not at rest but tends to rise, owing to the decreased density.) It may be assumed that the line of sight to the observer from the rod held down the grade is not affected abnormally while the sight to the rod held up the grade is usually close to the ground and must pass through the layers of lower density near the earth's surface. This sight would be less refracted than the one down the grade and may even be negatively refracted, therefore the morning running would give too small a difference between the zeroes of the rods sighted on from one station. It is the speaker's belief that, other things being equal, a line of levels run over steep grades in two directions in the afternoon, from noon to about one hour before sundown, will give results closer to the truth than levels with both runnings in the forenoon or with one leveling in the forenoon and the other in the afternoon. It is believed that this also applies to leveling over slopes of moderate grade.

The following table gives data for leveling done under different conditions of the sky. The letter C stands for cloudy and S for sunshine or clear.

RUNNINGS IN CLOUDY AND CLEAR WEATHER

	Steep Grades	Low Grades
Number of sections	56	217
C–S, total, positive	+ 159.2	+ 473.1
Mean discrepancy	+ 2.84	+ 2.18
Number of sections	45	228
C–S, total, negative	— 85.1	— 482.3
Mean discrepancy	— 1.89	— 2.12
Number of sections	101	445
Mean discrepancy	2.42	2.15
Accumulated discrepancy	+ 74.1	— 9.2
Mean accumulation per section	+ 0.73	— 0.02

The mean accumulated discrepancy here is + 0.73 for the steep grades while for the low grades it is practically zero.

It is the general belief among geodesists that the leveling under a cloudy sky is practically free from systematic errors resulting from atmospheric conditions. Therefore it would appear that the leveling under a clear sky causes the observed differences in elevation on steep grades to be too small.

In the following table are given data for the steep sections which had one running in clear and the other in cloudy weather, but the data are arranged in two groups, one where the running in sunshine was made in the morning called (SA) while the other has the running in sunshine made in the afternoon (SP):

RUNNINGS WHEN CLOUDY AND ON CLEAR MORNINGS

	Steep Grades	Low Grades
Number of sections	56	240
C–SA, accumulation per section	+ 0.24	+ 0.11

RUNNINGS WHEN CLOUDY AND ON CLEAR AFTERNOONS

	Steep Grades	Low Grades
Number of sections	45	215
C–SP, accumulation per section	+ 1.34	— 0.21

The number of sections on steep grades is too small to warrant our drawing any definite conclusions from the data given. The indication from the steep section is that the afternoon running gives a value lower than the morning value.

The average accumulated values of C–SA and C–SP for the sections with low grade are small, + 0.11 millimeter per section in the former and — 0.21 millimeter per section in the latter. These sections are quite numerous as compared with the number of steep sections, and should no doubt be given some consideration before coming to a decision as to whether the morning or afternoon runnings in sunshine give the larger differences.

The data in the following table show some relations between the systematic errors of leveling and calm and windy weather. The letter C stands for calm and W for wind.

RUNNINGS IN CALM AND IN WIND

	Steep Grades	Low Grades
Number of sections	63	277
C–W, total, positive	+ 140.0	+ 544.3
Mean discrepancy	+ 2.22	+ 2.0
Number of sections	75	345
C–W, total, negative	— 199.8	— 757.4
Mean discrepancy	— 2.66	— 2.20
Number of sections	138	622
Mean discrepancy	2.46	2.11
Accumulated discrepancy	— 59.8	— 203.1
Mean accumulation per section	— 0.43	— 0.33

Both for the steep and low grades the runnings during wind give on an average greater differences in elevation between the ends of a section than the runnings during calm. The average is almost as large for the low grades as for the steep ones. This can not be considered to be a general rule for other factors may and probably do influence the results. All of the lines are in the western portion of the United States where it is usually more windy in the afternoon than in the morning. Calm is infrequent there in the afternoon. Therefore the value of C–W would be somewhat confused with the value of P–A.

If both runnings are made in the forenoon or both in the afternoon, then the values of C–W should be practically free from the effect of the time of the day. In the following table there are given the data for such sections, the amount of grade not being considered.

SECTIONS WITH BOTH RUNNINGS IN THE MORNING OR BOTH IN THE AFTERNOON

Number of sections	85
C–W, total, positive	+ 177.2
Mean discrepancy	+ 2.08
Number of sections	90
C–W, total, negative	— 222.3
Mean discrepancy	— 2.47
Number of sections	175
Accumulated discrepancy	— 45.1
Mean accumulation per section	— 0.26

The effect of morning and afternoon conditions being eliminated (but not the cloudy or clear sky) we have a result which shows a larger value on an average for the running in wind than the one in calm.

There are 495 sections, each of which had one running in the morning and one running in the afternoon with both runnings made during calm. These sections should have values for *P–A* which are free from the effect of calm and wind. The data for these sections are shown below. The grade is not considered.

SECTIONS RUN IN BOTH DIRECTIONS DURING CALM

Number of sections	259
P–A, total, positive	+ 603.5
Mean discrepancy	+ 2.33
Number of sections	236
P–A, total, negative	— 497.5
Mean discrepancy	— 2.11
Number of sections	495
Accumulated discrepancy	+ 106.0
Mean accumulation per section	+ 0.21

The results in the above table are free from the effects of wind and calm, but may be and probably are somewhat affected by cloudy or clear sky. But the indication is that the afternoon running is greater than the forenoon, on an average.

If it is assumed that the running in wind is free from error, then the data for the sections shown below should give an indication as to whether an afternoon or forenoon running of a section will give the greater difference in elevation. The amount of grade is not considered.

The term $(C–W)A$ represents calm minus wind, with the calm running in the forenoon, while $(C–W)P$ is the same, except that the calm running is in the afternoon.

		Mm.
Number of sections 256, total positive value	(C–W) A	+ 499.2
Number of sections 330, total negative value	(C–W) A	— 759.0
Number of sections 94, total positive value	(C–W) P	+ 221.7
Number of sections 87, total negative value	(C–W) P	— 182.3
Mean accumulated discrepancy per section for	(C–W) A	— 0.44
	(C–W) P	+ 0.22

The indications from these data are that the difference from the calm running in the forenoon is too small and from the calm running in the afternoon too great, upon the assumption that the running in wind is without error. This bears out the conclusion stated earlier in this paper that the afternoon running gives a greater difference in elevation than the morning running.

CONCLUSIONS

While the data used in the investigation into the sources of error in precise leveling are not sufficient to warrant any definite statements, yet they seem to justify the following conclusions as probable.

1. The average size of the discrepancy between the values of the difference in elevation determined twice under different conditions does not give a clear idea of the magnitude of the accidental errors which may be produced by certain conditions, as the custom is to make the length of sight as great as the conditions will permit. Therefore, the extra length of sight may offset otherwise favorable conditions and give a large difference between two runnings of a section.

2. For sections run twice under different conditions the average accumulated value of the discrepancy is greater for the sections with steep grades than with low grades; the direction of the running being ignored and only the actual difference in elevation between the ends of a section being considered.

3. On all grades, but more especially the

steep ones, the difference in elevation determined in the afternoon is on an average greater than that determined in the forenoon.

4. On an average, a running during wind gives a greater difference in elevation than one during calm. The amount of this difference is somewhat greater for the steep than for the low grades.

5. On an average a running when the sky is cloudy gives a larger difference in elevation between two points, on a steep grade, than a running while the sun is shining. For low grades there is practically no difference, on an average, between the runnings under the two conditions.

6. For steep grades (about 10 meters per kilometer) the probability is that the afternoon running gives, on an average, a result closer to the truth than the forenoon running. The afternoon running should be ended sometime before sundown. The running in wind probably gives results on an average closer to the truth than a running in calm.

7. While the data in the tables given above make these conclusions justifiable, yet, owing to the fact that there are so many conditions to be considered, it is impracticable to obtain at present any reliable numerical values for the effect of any given atmospheric condition or set of conditions.

8. It is believed that, other things being equal, the running in the afternoon (if not within about an hour of sunset) gives, on an average, more accurate results than the forenoon running; also that, other things being equal, a running in wind is more accurate, on an average, than one in calm; and, that other things being equal, a running with a cloudy sky will be more accurate, on an average, than one in sunshine. Hence, the ideal condition would be an afternoon with a moderate wind and a cloudy sky.

9. It is believed that the mere fact of running backward or forward has no real effect on the result of a running, as the value of *B–F* may vary in sign for different lines and even for different parts of a single line.

William Bowie

U. S. Coast and Geodetic Survey

BESSEY HALL AT THE UNIVERSITY OF NEBRASKA

Dr. Bessey is gone, but he leaves with us an imperishable memory. He was the first professor in the natural science group to remain long with the University of Nebraska and to leave an indelible mark upon it. It is fitting that the permanent home of two fundamental natural sciences in the university should be named in his honor.

The writer believes that he first suggested naming such a building after Dr. Bessey when he penned for the approval page 21 of the biennial report of 1911–12. This report contains the sentence:

> The inadequate and dangerous building known as Nebraska Hall should be removed and an adequate building called Bessey Hall in honor of Dr. Bessey erected to house the natural sciences.

Nevertheless when he wrote these words it was then as now the writer's opinion that in general no building built at public expense should be named after the living. Dr. Bessey was great enough so that this exception was planned, but his lamented death prevented the exception being made. Let us now render his memory a special honor by resolving that hereafter no building shall be named for any one until his life's work is complete. This is in harmony with the regents' act in deciding that hereafter the title of head dean shall not be awarded.

Some of the special friends of Dr. Bessey are disappointed that the building is not to be located on a more conspicuous site. To these I would say that Dr. Bessey insisted on only one thing—north light for the use of his microscopes. He was, however, pleased to have the building located away from the noise and dust of heavy traffic. The location as now determined met his critical approval. The building will have north windows along its main side and will be so located that no other university building can by any possibility obstruct the view.

The building will have three stories above ground. The basement, not to be used for class-room purposes, will be utilized for lockers, toilet rooms, store rooms, constant tem-

perature rooms and other features of a modern laboratory of natural science. The entire space of the building will be divided almost equally between the departments of botany and zoology. Commodious offices and a number of special rooms for the use and comfort of the occupants will be provided. The building will be 235 feet long and 75 feet wide in its widest part. The center of the building facing south will open upon the space reserved for greenhouses. The building itself is to be of brick, hard burnt, of a reddish brown color, selected with a certain roughness and bloom on the surface. The mortar joints will be wide and raked. The trimming will be of Bedford stone. This material will also form the facing of the building as high as the base of the windows on the first floor.

The building itself is to be of the steel wall-bearing type and will be thoroughly fireproof. In this type of structure the masonry of the walls carries part of the weight of the frame while the frame itself supports the floors, partitions and roof.

In harmony with all the new buildings of the university the building will be of classic architecture. It will depend for its beauty on graceful lines and symmetry rather than on expensive ornamentation. In harmony with Dr. Bessey's character we shall try to make the building just as permanent as the building skill of the times through the use of brick, steel and concrete will permit. It ought to stand for 500 years at least. Further, in keeping with Dr. Bessey's character it will be attractive without ostentation, built for permanence and usefulness rather than show.

Aside from the sentiment connected with its erection it will be a building much needed by the university. Botany and zoology have never had adequate quarters here. This will house them in a way worthy of a great university. While these departments will have considerably more space than they have at present, we are not building large enough to care for their growth for many years to come. When the number of students of botany becomes too large for the new quarters, we can build a separate building for zoology, leaving the entire space of the Bessey building to botany.

The building will cost approximately $200,000 when completed and finished. The university considers itself fortunate in the fact that the lowest bid was made by a firm which has not only the reputation of doing excellent work but of doing its work promptly. This firm agrees to complete the general construction in 120 working days. Assuming, then, that there is not too much cold and stormy weather during the spring and summer following, there should be no difficulty in having the building ready for use at the beginning of the next school year. The ground is now ready so that the excavation may begin at any time.

For a few hundred dollars a beautiful memorial tablet could be placed in the principal hall of the new building. Some of the former students of Dr. Bessey have attained wealth. Would not some one of them like to volunteer to place a memorial worthy of his beloved teacher where the thousands of students that will throng this building in the years to come may look upon his features cast in enduring bronze by some skilled workman after the clay model of some great artist? The regents of the university will be pleased to accept and place in a suitable place such a tribute.

S. Avery

THE COLUMBUS MEETING OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

The sixty-eighth meeting of the American Association for the Advancement of Science, and the fourteenth of the "Convocation Week" meetings, will be held in Columbus, Ohio, from December 27, 1915, to January 1, 1916. Hotel Chittenden will be the headquarters.

The council will meet on Monday morning, December 27, and each following morning.

The opening general session of the association will be held at 8 o'clock P.M. on Monday, December 27, in the university chapel of university hall. The meeting will be called to order by the retiring president, Dr. Charles William Eliot, who will introduce the president of the meeting, Dr. William Wallace Campbell. Addresses of welcome will be made by President W. O. Thompson, of the Ohio

State University, and others to be announced, to which President Campbell will reply. The annual address of the retiring president, Dr. Charles W. Eliot, will then be given on "The Fruits, Prospects and Lessons of Recent Biological Science." Following the adjournment of the general session there will be a reception in the library, tendered by the Ohio State University, to members of the association and affiliated societies, with accompanying ladies.

The sections of the association and the special societies will hold their meetings through the week. Addresses of retiring vice-presidents of the association are as follows:

Vice-president H. S. White, before the Section of Mathematics and Astronomy: "Poncelet Polygons."

Vice-president Anthony Zeleny, before the Section of Physics: "The Dependence of Progress in Science on the Development of Instruments."

Vice-president F. R. Lillie, before the Section of Zoology: "The History of the Fertilization Problem."

Vice-president G. P. Clinton, before the Section of Botany: "Botany in Relation to American Agriculture."

Vice-president E. E. Rittenhouse, before the Section of Social and Economic Science: "Upbuilding American Vitality: the Need for a Scientific Investigation."

Vice-president R. M. Pearce, before the Section of Physiology and Experimental Medicine: "The Work and Opportunities of a University Department for Research in Medicine."

Vice-president P. H. Hanus, before the Section of Education: "City School Superintendents' Reports."

Vice-president L. H. Bailey, before the Section of Agriculture: "The Forthcoming Situation in Agricultural Work (Part II.)."

The following societies have indicated their intention to meet in Columbus during Convocation Week in affiliation with the American Association for the Advancement of Science:

American Society of Naturalists.—Meets on Thursday, December 30. Will hold symposium with American Society of Zoologists on Recent Advances in Fundamental Problems of Genetics. Annual dinner, same date, at 7 P.M., with presidential address by F. R. Lillie. Secretary, Dr. B. M. Davis, University of Pennsylvania, Philadelphia, Pa.

American Society of Zoologists.—Meets in joint session with Section F, A. A. A. S. Will hold symposium with American Society of Naturalists on Thursday, December 30, as above announced. President, William A. Locy. Secretary, Dr. Caswell Grave, Johns Hopkins University, Baltimore, Md.

Entomological Society of America.—Meets on Wednesday, Thursday and Friday, December 29, 30 and 31. Annual public address by Dr. C. Gordon Hewitt. Wednesday, December 29, at 8 P.M. President, V. L. Kellogg. Secretary, Professor Alex. D. MacGillivray, 603 West Michigan Ave., Urbana, Ill.

American Association of Economic Entomologists.—Meets on Monday, Tuesday and Wednesday, December 27 to 29. President, Glenn W. Herrick. Secretary, Albert F. Burgess, Melrose Highlands, Mass.

American Physical Society.—Meets on Tuesday, Wednesday and Thursday, December 28, 29 and 30, in joint session with Section B. President, Ernest Merritt. Secretary, Dr. Alfred D. Cole, Ohio State University, Columbus, Ohio.

Botanical Society of America.—Meets on Monday to Friday, December 27 to 31. Will hold joint meeting with Section G, A. A. A. S., on Tuesday afternoon, December 28, and with American Phytopathological Society on Thursday afternoon, December 30. Annual dinner for all botanists will be held Wednesday evening, December 29, at Hotel Hartman. President, John M. Coulter. Secretary, Harley H. Bartlett, Botanical Laboratory, University of Michigan, Ann Arbor, Mich.

American Phytopathological Society.—Meets on Tuesday to Friday, December 28 to 31. Will hold joint meeting with Section G, A. A. A. S., on Tuesday, December 28, and with Botanical Society of America on Thursday, December 30. President, H. H. Whetzel. Secretary, Dr. C. L. Shear, U. S. Department of Agriculture, Washington, D. C.

Botanists of the Central States—Will hold no separate meeting, but will present its papers in connection with Section G, A. A. A. S. Secretary, Dr. Edward A. Burt, Missouri Botanical Garden, St. Louis, Mo.

Society for Horticultural Science.—Meets on Tuesday to Thursday, December 28 to 30. President, W. L. Howard. Secretary, Professor C. P. Close, College Park, Maryland.

Association of Official Seed Analysts of North America.—Meets on Tuesday and Wednesday, December 28 and 29. President, W. L. Oswald. Secretary, John P. Heylar, Agricultural Experiment Station, New Brunswick, N. J.

American Microscopical Society.—Will hold executive committee luncheon on Tuesday, December 28, at 12:30 P.M., followed by business meeting, same date, at 5 P.M. President, C. A. Kofoid. Secretary, Professor T. W. Galloway, James Millikin University, Decatur, Ill.

American Mathematical Society (Chicago Section).—Meets on Thursday, Friday and Saturday, December 30 to January 1. Will hold joint meeting with Section A, A. A. A. S., on Thursday, December 30, at 2 P.M. Secretary, Chicago Section, Professor H. E. Slaught, 5548 Kenwood Avenue, Chicago, Ill.

American Federation of Teachers of the Mathematical and the Natural Sciences.—Will meet on dates to be announced. Secretary, Professor William A. Hedrick, McKinley Manual Training School, Washington, D. C.

American Nature-Study Society.—Meets on Thursday and Friday, December 30 and 31. Will hold joint session with School Garden Association of America on Thursday, December 30, at 2:30 P.M. President, L. H. Bailey. Secretary, Professor E. R. Downing, University of Chicago, Chicago, Ill.

School Garden Association of America.—Meets in joint session with American Nature-Study Society on Thursday, December 30, at 2:30 P.M. President, Van Evrie Kilpatrick, 124 West 30th St., New York, N. Y.

Society of the Sigma Xi.—Will meet on dates to be announced. President, Dr. Charles S. Howe, Case School of Applied Science, Cleveland, Ohio.

Wilson Ornithological Club.—Will meet on dates to be announced. President, T. C. Stephens, Morningside College, Sioux City, Iowa.

Gamma Alpha Graduate Scientific Fraternity.—Will meet on dates to be announced. Recorder, L. C. Johnson, 613 West Michigan Avenue, Urbana, Ill.

SCIENTIFIC NOTES AND NEWS

THE Nobel prize for physics for 1914, according to a Reuter dispatch from Stockholm, has been awarded to Professor Max von Laue, of Frankfort-on-Main, for his discovery of the diffraction of rays in crystals. The prize for chemistry for the same year has been awarded to Professor Theodore William Richards, of Harvard University, for fixing the atomic weights of chemical elements.

DR. WALLACE BUTTERICK, director of the China Medical Board, of the Rockefeller Foundation, Dr. Simon Flexner, of the Rockefeller Institute for Medical Research, and Dr. William H. Welch, professor of pathology at Johns Hopkins University, were the guests of honor at the recent dedication of the new Sleeper-Davis Memorial Hospital, Peking, China. The new building is a five-story structure erected by the Methodist-Episcopal Church at a cost of $180,000.

THE Romanes lecture before the University of Oxford will be delivered this year by Professor E. B. Poulton, Hope professor of zoology in the university, on December 7. The subject will be "Science and the Great War."

THE honorary degree of D.Sc. was conferred on October 26 on Mr. Guy A. K. Marshall, director of the recently established Imperial Bureau of Entomology.

DR. J. HORNE has been elected president of the Royal Society of Edinburgh. The vice-presidents of the Royal Society are: Professor F. O. Bower, Sir T. R. Fraser, Dr. B. N. Peach, Sir E. A. Schäfer, the Right Hon. Sir J. H. A. Macdonald and Professor R. A. Sampson.

THE Berlin Geographical Society has elected General von Beseler as its president.

DR. SVEN VON HEDIN has been elected a corresponding member of the Vienna Academy of Sciences.

WE learn from *Nature* that Mr. W. Marriott has retired from the post of assistant secretary of the Royal Meteorological Society held by him for the last forty years, and has been succeeded by Mr. A. H. Brown, the chief clerk of the society.

DR. HERMAN FISCHER, formerly professor of surgery in Breslau, has celebrated his eighty-fifth birthday.

DR. ALFRED WERNER has been made a member of the *Reichsanstalt*, Berlin.

DR. KARL H. VAN NORMAN, first assistant superintendent of Johns Hopkins Hospital, has resigned to accept a captaincy in a Canadian regiment.

PROFESSOR F. J. ALWAY, of the department of chemistry of soils of the University of Minnesota, has been elected president of the Min-

nesota section of the American Chemical Sosiety.

Dr. Clark Wissler and Dr. Robert H. Lowie, of the American Museum of Natural History, have been appointed delegates from the New York Academy of Sciences to the Nineteenth International Congress of Americanists which meets in Washington at the end of December.

It is stated in *Nature* that in addition to the awards announced in April for papers read at the meetings, the council of the Institution of Civil Engineers has made the following awards for papers published in the *Proceedings* during the session 1914–15: A Telford gold medal to Mr. James Forgie (New York); Telford premiums to Messrs. J. R. Mason (Dunedin, N. Z.), Harold Berridge (Aden), C. R. White (London), C. S. Churchill (Roanoke, Va.), and the Trevithick premium to Mr. A. Poulson (Lemvig, Denmark). The Indian premium for 1915 has been awarded to Mr. C. W. Anderson (Midnapore, India). The ninety-seventh session of the Institution was opened on November 2, when Mr. Alexander Ross, president, delivered an address and presented the awards.

Mr. Frank C. Baker, until recently acting director of the Chicago Academy of Sciences, now zoological investigator for the New York State College of Forestry at Syracuse University, gave a popular illustrated lecture in the lecture course of the Syracuse Chapter of Sigma Xi on November 8 on "Hunting Birds with a Camera."

Robert Allyn Budington, professor of zoology in Oberlin College, lectured recently on "Some of the Results of Biological Study," at Goucher College, where the department of biology has recently moved into new and enlarged laboratories.

The general meeting of the Röntgen Society, London, was held on October 4, at the Institution of Electrical Engineers, when the president, Mr. J. H. Gardiner, delivered an address and new apparatus was exhibited.

Dr. Charles F. Chandler, professor emeritus of Columbia University, has given for the department of arts and sciences of the University three lectures on "The Art of Photography."

The twenty-third summer meeting of the American Mathematical Society will be held at Harvard University early in September, 1916. At the eighth colloquium of the society, held in connection with this meeting, courses of lectures will be delivered as follows: By Professor G. C. Evans: "Topics from the Theory and Applications of Functionals, including Integral Equations." By Professor Oswald Veblen: "Analysis Situs."

Edward Lee Greene, associate in botany at the Smithsonian Institution since 1904, recently elected head of the botanical department of Notre Dame University at South Bend, Ind., died in Washington on November 10, aged seventy-two years. From 1885 to 1895 Dr. Greene was professor of botany in the University of California, and from 1895 to 1904 in the Catholic University of America.

Sir Andrew Noble, F.R.S., distinguished for his scientific work on artillery and explosives, died on October 22, at eighty-four years of age.

Professor Vivian B. Lewis, until last year professor of chemistry in the Royal Naval College, died on October 23, aged sixty-three years.

Dr. R. Assheton, F.R.S., university lecturer in animal embryology at the University of Cambridge, died on October 23, aged fifty-one years.

Dr. Ernst Werner Maria von Olfers, known for his work in sanitation, has died at Königsberg in his seventy-fifth year.

The sequence of events so often observed in the history of gold-mining camps has been repeated in the Willow Creek district, Alaska. The earliest prospectors, in 1897, were primarily interested in the search for placer gold and having found it were too busily engaged in mining to trace the stream gold to the veins from which it originally came. It was nearly ten years later that the first of the valuable quartz veins that now yield most of the gold mined in the district was discovered. Since,

1906, however, quartz mining has progressed steadily and has rested upon a substantial basis. In 1913 the production of the district for the first time exceeded $100,000, but in 1914 it was almost treble that amount. Three mills are in operation, and more will soon be installed. With the increase in the depth of mining the veins show no diminution in the amount or tenor of the gold. A study of the geologic conditions in this general area leads to the conclusion that veins similar to those now worked may be found beyond the borders of the present mining district, and prospects already being developed confirm this conclusion. The district lies near the route of the government railroad from Seward to Fairbanks, and the cheaper transportation should greatly stimulate its development. There is thus every indication that the Willow Creek district will steadily increase in importance as a gold-mining camp and that it will have a long period of productiveness. Since the district's establishment as a gold quartz mining camp the gold placers which originally were regarded as the only valuable gold deposits, have decreased in importance until their annual output is now small, yet under the more favorable conditions of transportation soon to be realized it is possible that placer mining may again be profitably carried on. A report on the Willow Creek district by S. R. Capps, published as Bulletin 607 of the United States Geological Survey, includes in addition to a description of the mines and prospects, a discussion of the history, geography and geology of the district. The report is illustrated by a topographic and a geologic map on a scale of about 1 inch to the mile, and by numerous photographs and text-figures.

A PHASE of the study of the underground waters of southern Louisiana is their utilization in the cultivation of rice by irrigation. In 1888 lowlands near the bayous suitable for growing sugar cane, corn and cotton could be purchased for $3.50 an acre, and the prairie lands back from the bayous could be bought for $1 an acre. With almost the first crop under irrigation, however, the values showed a marked rise and have continued to increase. In the first five years the value of the best rice lands rose to $10 an acre, and soon after that it rose to $30 and even $50 an acre. The first people to plant rice in southern Louisiana, according to the United States Geological Survey, were the Acadians, who, after their expulsion from Nova Scotia by the English in 1755, settled in considerable numbers in Louisiana. Their cultivation of rice, almost absolutely primitive in its methods, was confined to the lowlands along the bayous, the prairies affording pasturage for the Acadians' herds of cattle. Few of the lowland areas admitted of satisfactory drainage, and they were too small for profitable cultivation. The crops frequently failed in years of deficient rainfall. Attempts were made to create additional water supplies by building levees across low sags or coulees at points higher than the cultivated areas, but generally either the rainfall proved deficient or the reservoirs were too small. Little advance was made over the Acadian methods until very recently. Experiments in unusually wet years had shown that the soils of the prairies were adapted to the growth of rice if sufficient water was at hand. This led to the trial of pumps as a means of raising water from the bayous to the rice fields. So successful was the test that pumps were at once installed at many points, and in a few years tens of thousands of acres of previously almost useless land, lying 10 to 70 feet above the bayous, were put under cultivation. The first large pump was installed in 1894 on the Bayou Plaquemine, in Acadia Parish, near Crowley. Although its failure at a critical time involved the partial loss of the crop, it showed that rice could be cultivated by pumping, which has been gradually adopted on larger and larger scales until now in the larger plants batteries of pumps operated by compound Corliss engines are in common use.

THE archeological work carried on in Manitoba for the Geological Survey, Canada, by Mr. W. B. Nickerson, has been completed for the season. An artificial mound was found on the most conspicuous headland overlooking the Assiniboine River about six miles north of Alexander. This Mr. Nickerson explored and found to be a burial mound. Among the finds were one hundred and sixty-two marine shells

and six cylindrical objects—beads or pendants—made of the columella of the conch. They indicate trade or expedition as far as the sea. Two groups, each of more than one hundred gravel mounds, on terraces in the Assiniboine Valley, were found to be of natural origin, although resembling artificial burial mounds in appearance. No mounds were found in the valley of the Little Saskatchewan, and slight evidence of habitation. Near Arden, Mr. Nickerson explored a long mound, consisting of two dome-shaped ends with a connecting grade, and a broad, dome-shaped mound, in which were found parts of three human skeletons, a perforated disc made of shell, and two objects made of bone, probably used as bracelets. A third mound, within the village of Arden, had been previously disturbed. Several camp sites were found at the foot of the Assiniboine Hills at springs forming small streams, also in the vicinity of Arden, along the White Mud River. Mr. Nickerson took seventy-five photographic films in connection with this work and secured a number of gifts for the Dominion collections.

UNIVERSITY AND EDUCATIONAL NEWS

As was noted in SCIENCE last week, Columbia University received by the will of Amos F. Eno the residuary estate. It also receives a revisionary interest in certain bequests. In addition, the General Society of Mechanics and Tradesmen receives $1,800,000, and bequests of $250,000 each are made to New York University, The American Museum of Natural History, the Metropolitan Museum of Art and the New York Association for Improving the Condition of the Poor.

MR. AND MRS. NORMAN W. HARRIS, of Chicago, have increased their gift of $25,000 to Mount Holyoke College made at the time of the seventy-fifth anniversary, to $50,000, for the endowment of the chair of zoology. Mrs. Harris is a graduate of the college of the class of 1870.

THE date for the dedication of the new buildings of the Massachusetts Institute of Technology has been fixed by the executive committee of the corporation for June 14, 1916. Practically all the stonework of the buildings has been completed and nearly all the carving, which in addition to the decorative features of capital, cornice and portico, will include the names of the founders of science incised about the towers. In the interior the floors are in process of finishing, this being done by means of electric polishers, which are carrying on the work at the rate of 2,500 square feet a day. The rough plumbing is practically all in place and the installation of fixtures is under way. In ten of the buildings the steam heating system is ready and later this month, when the boiler house is completed, the buildings will be dried out by steam heat.

AT the University of Minnesota efforts are being made to bring faculty and regents into closer personal relations. At a general assembly of the whole teaching staff held September 27 ten of the twelve regents of the university were present and made brief addresses. The president of the board, Mr. Fred B. Snyder, emphasized the fact that the regents regarded the faculty members not as employees but as colleagues responsible for the really important work of the university. He made an appeal for the hearty cooperation of all concerned for the welfare of the institution. On the evening of November 3 the new members of the faculty were invited to meet the regents at the house of the president of the university. For December 14 a dinner is being arranged by a faculty committee. On this occasion there will be an informal discussion in which it is expected that both faculty and regents will express their views about university ideals and policies.

PROFESSOR T. W. GALLOWAY, Ph.D., who has occupied the chair of biology at James Millikin University at Decatur, Ill., since the establishment of that institution in 1903, has been appointed professor of zoology at Beloit College, Beloit, Wisconsin. A. A. Tyler, Ph.D. (Columbia, '97), for some years professor of biology in Bellevue College, Omaha, Nebraska, has been appointed to the chair of biology at

James Millikin University, to succeed Dr. Galloway.

J. A. MOYER, professor of mechanical engineering in the Pennsylvania State College and director of the college extension work, has been appointed by Governor Walsh to the directorship of the extension service which is to be organized in Massachusetts.

JAMES KENDALL, D.S., has been promoted to be assistant professor of chemistry in Columbia University.

DR. L. G. ROWNTREE, of the department of medicine of Johns Hopkins University, has been elected professor of medicine and chief of the department of medicine in the University of Minnesota Medical School. Dr. Rowntree will devote practically his entire time to the service of the medical school, although he will have the privilege of seeing a limited number of patients who may be referred to him by physicians.

AT the University of Michigan, Junior Professors Peter Field, L. C. Karpinski and T. R. Running have been promoted to associate professorships of mathematics. Drs. Tomlinson Fort and T. H. Hilderbrandt have been promoted from instructorships to assistant professorships of mathematics. Dr. A. L. Nelson has been appointed instructor in mathematics.

DISCUSSION AND CORRESPONDENCE

THE PUBLICATION OF NEW SPECIES

IN these days when taxonomic literature has reached such enormous proportions and is growing so rapidly that even the specialist has difficulty in keeping up with the literature of his own particular group, it seems to me that the interests of science would be better subserved by the use of greater care in selecting the medium of publication of new species. The pages of such general magazines as SCIENCE should be devoted to papers of general interest to the scientific, and to scientific papers of a nature unsuited to the special periodicals. For example, with a magazine in America devoted exclusively to Mollusca, why should an occasional new species of mollusk be published in SCIENCE, thus compelling the student of mollusks to search the files of that bulky magazine in order to be sure of missing nothing in his systematic work? Why not send it to a magazine especially devoted to the subject? With several excellent bird magazines in the United States, why should a technical discussion of the taxonomic status of a bird species appear in SCIENCE? With magazines exclusively devoted to botany, why should a new species of plant found in Colorado be published in an annual report of an experiment station in a far distant state, a volume in which surely no botanist could be expected to look for such a description if he were working upon the plants of that particular group or that particular region? Are not the difficulties of systematic botanical and zoological work great enough without vastly enhancing them by scattering the descriptions of new species? The examples above given are mere samples of scores of similar instances which come to our attention every year, to the discouragement of hard-worked students, and especially those remote from very large libraries. Furthermore, there are altogether too many ephemeral publications of small educational institutions and local scientific societies, having very limited circulation, but publishing strictly taxonomic papers which often fail to reach the attention of specialists for years, and then suddenly bob up to cause confusion in nomenclature. To make matters worse, descriptions of new species sometimes appear in leaflets or small pamphlets, published privately or by some small institution or society and not forming part of any series into which they would be finally bound and thus preserved. What happens to such a leaflet when it reaches a library? Is it not usually lost? Is it likely to be easily available to the student of ten or twenty years hence, as it would be if published in *The Nautilus,* or *The Auk,* or *The Botanical Gazette,* or even in *Nature* or SCIENCE? In how many libraries may a student be able to find it in fifteen years? Although many new species are described at the University of Colorado, that institution has wisely excluded all such descriptions from its *Studies* and *Bulletin,* taking the position that they should appear in

periodicals especially devoted to the particular subjects, or in publications which have wide circulation and are well known to habitually publish such papers. If all publishers and naturalists would take the same position it would surely greatly simplify the work of the future systematist.

JUNIUS HENDERSON

THE EFFECT OF CYANIDE ON THE LOCUST-BORER AND THE LOCUST-TREE

DURING the past five years a number of experiments have been made from the office of the Illinois state entomologist with methods for destroying the black locust-borer (*Cyllene robinæ*). From articles appearing in SCIENCE during the last few months, especially those by Professor Fernando Sanford in the issue of October 9, 1914, and by Professor C. H. Shattuck in the issue of February 26, 1915, it seemed probable that at least a part of the borers in infested locust-trees might be killed by introducing small amounts of potassium cyanide into the trunk and bark.

Early in the spring of 1915, fifty black locust-trees, fourteen in a small grove at Athens, in central Illinois, and thirty-six in a large plantation at Union Grove, in northwestern Illinois, were treated with potassium cyanide and sodium cyanide in the following manner:

The trees selected were from one to seven inches in diameter and were nearly all badly infested with the larvæ of the locust-borer. The borers were still in their overwintering cells in the bark, but were just becoming active at the time. The cyanide was placed in the trees in auger-holes of one fourth, one half, three fourths, and one inch diameter, bored at different heights from the ground and different depths into the trunk. The amounts of cyanide used in single trees varied from one twentieth to one half an ounce. The chemicals used were potassium cyanide, 98 per cent. pure, in small lumps, and cyanide-chloride carbonate mixture in granular form, guaranteed to contain 35–38 per cent. sodium cyanide. After the cyanide had been placed in the trees, the auger-holes were tightly plugged with corks driven in with a hammer.

The fourteen trees at Athens were treated March 12, and the thirty-six trees at Union Grove, April 1, 1915. The results of the treatments were taken at Union Grove July 13 and at Athens July 15, 1915.

The results of this experiment showed no benefit by the treatment. Of the fifty trees treated, eight could not be located in the summer, owing to the dense growth of weeds and sprouts. The treatment of these eight trees did not differ materially from that given the forty-two examined, and could not have made any marked difference in the results. Of the forty-two trees examined in July, twenty-three were dead and nineteen alive. Of the nineteen living trees, all but three contained living larvæ of *Cyllene robinæ*. In several cases living borers were found directly above and within six inches of the auger-holes, and in three cases the borers were within one inch of the auger-holes. Not only were the borers alive in the living trees, but in all cases where the trees had put forth leaves in the spring of 1915, living borers were present in numbers in the trunks, and could be found around the bases of the trunks of many of the trees that had not shown foliage the past spring. Not a single dead borer was found near the points where the cyanide had been placed.

While over half of the trees treated were dead, this was not entirely due to the effects of the cyanide, as at least twenty-five per cent. of the untreated trees in both groves had died from the effects of borer injuries. There can be no doubt, however, that the cyanide had a very injurious effect on the trees, as in all the living trees the bark was dead and the wood discolored for a greater or less distance around the holes where the cyanide had been placed.

It was an interesting fact, which has, however, no bearing on the effect of the cyanide on the trees, that some rodents, probably rabbits, had gnawed many of the trees around the auger-holes, deeply scoring the wood. There was no residue from the cyanide in any of the auger-holes when examined in July, whether the corks had been removed or not, and no odor of the cyanide could be detected in the wood.

No chemical tests were made for the presence of cyanide.

WESLEY P. FLINT
STATE ENTOMOLOGIST'S OFFICE,
URBANA, ILL.,
August 6, 1915

A NEW MITOTIC STRUCTURE

IN the *Journal of the Royal Microscopical Society,* April, 1915, Mr. E. Sheppard, F.R.M.S., published a paper entitled "A New Mitotic Structure Disclosed as the Result of New Technique." He describes at the ends of the dividing chromosomes "bead-like chromatin extensions" where the spindle fibers are attached. I want to draw his attention to the fact that these structures are well known to cytologists and that there is no special technique needed for their disclosure. My own experience is that they are most extremely developed in the maturation divisions of Trematodes. I have figured them in my paper "Die Chromatinreifung der Geschlechtszellen des Zoogonus mirus, etc.," *Arch. Zellforschg.,* Vol. 2, 1908. Better figures are found in Grégoire's publication, based on the same slides "La réduction dans le Zoogonus mirus, etc.," *La Cellule,* 25, 1909. He calls these structures "renflement d'insertion." For *Fasciola hepatica* they are described by A. Schellenberg, "Ovogenese, Eireifung und Befruchtung von *Fasciola hepatica* Arch. Zellforschg.," Vol. 6, 1910, and I know their presence in some other trematodes.

R. GOLDSCHMIDT

A METHOD OF MAINTAINING A SUPPLY OF PROTOZOA FOR LABORATORY USE

ONE of the difficulties that confront the teacher of elementary biology, especially in those institutions where a large number of students must be provided for, is that of obtaining a satisfactory supply of protozoa, especially of such forms as *Ameba, Euglena* and *Paramecium.* I have overcome this difficulty in such a simple manner that it may be worth while to state briefly how I keep a supply of these forms on hand. Four years ago I obtained from a pond some water and rubbish in which were present a few individuals of *Ameba, Euglena* and *Paramecium.* I prepared a culture made by boiling a handful of hay in about a half-gallon of water until the liquid assumed a dark brown color. This with a part of the hay was placed in a two-quart, cylindrical battery jar and permitted to stand open in the laboratory for twenty-four hours. The jar was then covered loosely with a pane of glass and set aside till bacteria had formed a scum over the surface of the liquid. The pond water and rubbish were then added and the jar still covered was set in a north window of the laboratory.

In a short time an abundance of *Paramecia* was present in the culture. The *Euglenæ* and *Amebæ* multiplied more slowly, but at the end of six months the jar was swarming with these two forms, while the *Paramecia* had decreased in number and were to be found chiefly at the bottom of the jar. Such a culture will usually afford a good supply for a year but I prepare a new culture every six months and stock it from the old one. By this method I have for the past four years kept on hand an abundant supply of these protozoa without going outside of my laboratory. At the opening of college I have on hand a culture newly prepared, in order to have an abundance of *Paramecia,* a second culture six months old and a third one year old. The hay infusion and the decomposing vegetable matter in the jar seem to furnish suitable food for the bacteria and *Euglena; Paramecium* feeds on the bacteria and *Ameba* on the encysted *Euglena.* Rotifers and a host of other protozoan forms abound in the cultures but the three forms most used in laboratory exercises are always present in abundance. In my laboratory I find it necessary to keep the culture in a north window; direct sunlight is not only not necessary but decidedly harmful, due probably to the heat rather than the light.

J. B. PARKER
BIOLOGICAL LABORATORY,
CATHOLIC UNIVERSITY OF AMERICA

QUOTATIONS

SCIENCE IN NATIONAL AFFAIRS

WE printed last week a valuable address by Professor J. A. Fleming on "Science in the

War and after the War." Though the address was an introductory lecture at University College, London, and was open to the public without fee or ticket, only the briefest mention of it appeared in the periodical press, and the points of national importance dealt with in it were unrecorded, except in our columns, in which it was our privilege to publish the address almost in full. We understand, of course, that the demands made upon the space available in the daily papers are many and insistent, yet we should have supposed that during the progress of a war in which victory will depend as much upon science and machinery as upon men, a summary of some of the points made by a leading authority upon applied science would be of greater public interest and importance than much of the unsubstantial chatter with which we are supplied daily.

In the course of his address, Professor Fleming himself supplied a reason for the neglect of scientific aspects of national affairs, in comparison with the attention given to the superficial views of politicians and other publicists. While success in science is measured solely by discovery of facts or relationships, in politics and public life generally it is secured by fluent speech and facile pen. In scientific work attention must be concentrated upon material fact, but the politician and the writer attach greater importance to persuasive words and phrases, and by their oratory or literary style are able to exert an influence upon public affairs altogether out of proportion to their position as determined by true standards of national value. Power, as regards government of the affairs of the nation, does not come from knowledge, but from dialectics: it is the lawyer who rules, with mind obsessed by the virtues of precedent and expediency, and to him men of science and inventors are but hewers of wood and drawers of water.

Under a democratic constitution it is perhaps too much to expect that Parliament will pay much attention to scientific men or methods; yet, as was shown in the debate upon the scheme for the institution of an advisory council of scientific and industrial research last May, the members of the House of Commons are ready to support plans for bringing science in closer connection with industry. The monies provided by Parliament for this purpose are to be under the control of a committee of the Privy Council, which will be advised by a council constituted of scientific and industrial experts. The scheme was conceived rightly enough, but when it passed into the hands of officials of the Board of Education much of its early promise was lost. Most people would regard it as essential that the executive officers of a council concerned with the promotion of industrial research should know what is done in this direction in other countries, and have sufficient knowledge of science and industry to formulate profitable schemes of work. The success of such a body depends largely upon the initiative of the secretary; and in an active and effective council we should expect him to be selected because of close acquaintance with problems of industrial development along scientific lines. But what is the position in this case? The scheme is issued by the president of the Board of Education, Mr. Arthur Henderson, a labor member, who owes his post entirely to political exigencies, the secretary of the committee of the privy council is the secretary of the board, Sir Amherst Selby-Bigge, whose amiability is above reproach, but who knows no more of practical science and technology than a schoolboy, and the secretary of the advisory council is Dr. H. F. Heath, whose interests are similarly in other fields than those of science.

The belief that the expert—whether scientific or industrial—has to be controlled or guided by permanent officials having no special knowledge of the particular subject in hand is typical of our executive system. While such a state of things exists, most of the advantages of enlisting men of science for national services must remain unfulfilled. The various scientific committees which have been appointed recently have, we believe, been able to give valuable aid in connection with problems submitted to them, but they would be far more effective if the chiefs of the departments

with which they are associated possessed a practical knowledge of scientific work and methods. Without such experience the executive is at the mercy of every assertive paradoxer and can not discriminate between impracticable devices and the judgment of science upon them. While, therefore, the country has at its disposal the work—either voluntary or nearly so—of experts in all branches of applied science, it can not use these services to the best advantage unless the departments concerned with them have scientific men among the permanent officials; and that is not the case at present.

The unbusinesslike methods of government departments have received severe criticism lately, but nothing has been said about the unscientific method of appointing committees of experts without well-qualified officers to direct or coordinate their work. The reason is that, with scarcely an exception, no daily paper has any one on its staff possessing the most elementary knowledge of the meaning of scientific research. Our guides and counsellors, both on the political platform and in the periodical press, can scarcely be expected to interest themselves greatly in subjects beyond their mental horizon, so when scientific matters are involved they confine themselves to a few platitudes, or say nothing at all. They are unable to distinguish a quack from a leading authority in science, and prefer to exercise their imaginations upon sensational announcements, rather than discuss the possibilities of sober scientific discovery. In all that relates to the interests of science—and that means in the end the interests of the nation—the men who influence public opinion and control the public services are mostly unenlightened and therefore unsympathetic.

The tacit assumption that public committees or departments concerned with scientific problems must have at their head officers of the army, navy, or civil service is responsible for delay in taking advantage of available expert knowledge and for the neglect to make effective use of science in national affairs, whether in times of war or peace. Just as a member of the government may serve in turn as president of the Board of Education, Board of Agriculture, Board of Trade, or any other department, without possessing any special qualifications to comprehend the work of either, so a public official may be placed in a position to dominate activities of which he can not understand the significance. Some day we hope that this mad system will be swept away, and that the men who exert control in all government offices will be those whose training or experience make them most capable of doing so effectively.—*Nature.*

SCIENTIFIC BOOKS

A Budget of Paradoxes. By AUGUSTUS DE MORGAN. Reprinted, with the author's additions, from the *Athenæum.* Second edition edited by DAVID EUGENE SMITH. Two volumes, I., viii + 402 pp.; II., 387 pp. The Open Court Publishing Co., 1915. Price $3.50 per volume.

The similarity between the work of David Eugene Smith and Augustus De Morgan in the field of popularizing mathematics has long been familiar to students of the history of science. This similarity has extended to many details; both men have participated in the publication of elementary text-books of excellence, both are known as editors of the mathematical department of encyclopedias and dictionaries, both have been energetic collectors of mathematical books and other mathematical material, and both have been distinguished by a wide and human interest in all phases of mathematical development. Hence it is eminently fitting that as editor of this new edition of "A Budget of Paradoxes" we should have Professor Smith, who not long ago continued so ably in the "Rara Arithmetica," De Morgan's bibliographical work, represented by "Arithmetical Books from the Invention of Printing to the Present Time" (London, 1847).

The first question which occurs to the casual reader whose eye catches the title is regarding the significance of the word "paradox." De Morgan answers this [I., 2] in a manner that even to-day has meaning for many who publish books. "A great many individuals, ever since

the rise of the mathematical method, have, each for himself, attacked its direct and indirect consequences. I shall not here stop to point out how the very accuracy of exact science gives better aim than the preceding state of things could give. I shall call each of these persons a *paradoxer,* and his system a *paradox.* I use the word in the old sense: a paradox is something which is apart from general opinion, either in subject-matter, method or conclusion." Further on in his introductory remarks De Morgan adds: "After looking into books of paradoxes for more than thirty years, and holding conversation with many persons who have written them, and many who might have done so, there is one point on which my mind is fully made up. The manner in which a paradoxer will show himself, as to sense or nonsense, will not depend upon what he maintains, but upon whether he has or has not made a sufficient knowledge of what has been done by others, *especially as to the mode of doing it,* a preliminary to inventing knowledge for himself. That a little knowledge is a dangerous thing is one of the most fallacious of proverbs. A person of small knowledge is in danger of trying to make his *little* do the work of *more;* but a person without any is in more danger of making his *no* knowledge do the work of *some.*"

How De Morgan would have enjoyed for his collection the solution (?) of Fermat's problem by Miss ————, of the New York schools, whose name will not go down in history, published by the staidest of New York evening papers; this problem to solve, or prove not solvable, $x^n + y^n = z^n$ in integers for n greater than 2 has been the subject of many similar solutions and the Wolfskehl prize of $25,000 has often been claimed and as often denied. Without fear of contradiction we may say that the final solution will be given by some able student of number theory who is not ignorant of "what has been done by others." Equally would De Morgan have welcomed the high-school boy's solution (?) of the trisection of an angle, with ruler and compass, published only three or four years ago in a journal devoted to elementary science. Particularly, too, De Morgan would have desired for his "Budget" something typical concerning our American prodigies, whose names, we note, are found more often in paragraphs than in monographs, more often in headlines than in footnotes.

How many works of to-day come within the classification of paradoxical nonsense, foisted upon the press by authors ignorant of "what has been done by others" in the fields in which these authors would instruct the public. Among these "paradoxers" are scientists of real fame in science, but without philosophy, who wish to instruct philosophers in philosophy, philosophers ignorant of the work of Georg Cantor and Dedekind who wish to instruct mathematicians about the nature of the number idea and the psychology of number, school superintendents who are profoundly ignorant of the fundamental ideas of arithmetic who wish to write text-books on arithmetic, old maids living in a two-room flat on the fifteenth floor of a New York apartment who wish to instruct the parents of the United States on the art of bringing up a large family of children, manufacturers successful in business who yearn to instruct the world in philosophy and science. These are modern *paradoxers* of the nonsense type who need another De Morgan to call attention to their folly.

"All the men who are now called discoverers, in every matter ruled by thought, have been men versed in the minds of their predecessors, and learned in what had been before them. There is not one exception. I do not say that every man has made direct acquaintance with the whole of his mental ancestry; many have, as I say, only known their grandfathers by the report of their fathers. But even on this point it is remarkable how many of the greatest names in all departments of knowledge have been real antiquaries in their several subjects.

". . . if any one will undertake to show a person of little or no knowledge who has established himself in a great matter of pure thought, let him bring forward his man and we shall see."

Let every editor have a copy of these words to enclose with rejected manuscripts which violate the principles so sanely laid down by De Morgan.

Mathematical paradoxes are largely connected with the squaring of the circle, the ratio π, the duplication of the cube, the trisection of the angle, and the number of the beast; astronomical paradoxes are quite as frequent, here, as the mathematical; religion, philosophy and medicine, too, enter in for a goodly share of attention. De Morgan had a very live interest in the history of science, and this interest finds frequent expression in the "Budget."

Not all the material, by any means, of these interesting volumes is concerned with paradoxes of the *nonsense* type. Le Verrier's planet Neptune is presented with certain original documents connected with the discovery; the names of Herschel, royal astronomer, and Brünnow, who was later director of the observatory at the University of Michigan, and Challis of the Cambridge Observatory are indications of a paradox, "something contrary to the current opinion" which was really revolutionary. Historical material appears with relative frequency, giving pleasant intervals of relief from regarding the errors of mankind.

Of particular interest are those notes which De Morgan inserts about men and affairs of his own time. The liberal footnotes added largely by Professor Smith, and occasionally by De Morgan's wife or from De Morgan's notes, contribute much to the modern reader's pleasure in perusing the volumes.

The "Budget," it need hardly be stated, was not intended to be read as a romance, although much of the material suggests that artistic rambling which is so delightfully characteristic of William De Morgan, the son of our mathematician Augustus De Morgan. Rather these are volumes to be read at odd moments, and always one will find profitable enjoyment. In spite of the interest and amusement with which we thumb the pages a feeling of sadness for the human frailty comes over the reader. De Morgan expresses this sentiment, too, in the brief but pointed comment on the work of an angle-trisector. After giving the title of the work De Morgan continues with a quotation of words from the author of the trisection, followed by five words of comment: "'The consequence of years of intense thought': very likely, and very sad."

The physical make-up of this edition is up to the high standard which has been set by other publications of the Open Court Company. In every way the reader who takes these volumes in hand has pleasure in store; we commend the works to all who take a kindly interest not only in the greatness but equally in the frailty of their fellows.

LOUIS C. KARPINSKI
UNIVERSITY OF MICHIGAN

Scottish National Antarctic Expedition. Report on the Scientific Results of the Voyage of the Scotia, during the Years 1902–4. Vol. IV., Zoology. Parts II.–XX., Vertebrates. Edinburgh, 1915. Pp. 505. 4to. 62 pl. 31 text-figures and 2 maps.

Before the voyage of the *Scotia* under the leadership of Dr. Wm. S. Bruce there had not been a deep-sea sounding taken south of S. Latitude 40° in the Atlantic Ocean. The uninviting lands of the South Orkneys, the South Shetlands and South Georgia, were rarely visited and relatively little known. As a direct result of the Swedish and Scottish expeditions in the Weddell Sea an extensive whale fishery has been developed having its headquarters at Leith. Now according to Dr. Bruce over a thousand people live in South Georgia, and during the summer months the South Orkneys and South Shetlands are a hive of industry, and altogether over five million dollars gross annual revenue is now taken in those regions previously regarded as worthless by business men.

Owing to the struggle in which the country is engaged, money for the publication of the scientific results could no longer be supplied by the treasury; and several of the reports were consequently issued in scientific periodicals, such as the *Ibis* and the *Proceedings of the Royal Physical Society;* but by the generosity of Sir Thomas Coats, the collaboration of the Scottish Oceanographical Society, the Carnegie Trust, the Royal Societies of Edinburgh and London and other friends and organizations, these and other papers are brought together in

this volume and illustrated in satisfactory form. Fifteen British naturalists have contributed papers, and the book is perhaps the most complete treatise on the Antarctic vertebrate fauna yet published.

The birds, seals, whales and fishes, are fully illustrated with excellent plates, and much space is given to anatomy, osteology and embryology; while the economic aspects of the fauna are not neglected. Papers on the tunicates and *Cephalodiscus* are included. A few forms obtained on the voyage but which are not strictly Antarctic are incidentally noticed. Altogether the members of the staff and the contributors to the explorations and publication of the results may justly congratulate themselves on the appearance of this handsome volume at a time when general attention is unfortunately diverted from matters of science and focused on the preservation of the empire.

WM. H. DALL

SPECIAL ARTICLES

THE CALCULATION OF TOTAL SALT CONTENT AND OF SPECIFIC GRAVITY IN MARINE WATERS[1]

To the investigator engaged in biological studies on marine problems, it is often desirable to ascertain the concentration of sea-water in terms capable of correlation with life phenomena. Such concentration records usually take the form of density determinations made with some standard type of densimeter at the prevailing temperature. These density readings, while useful as physical records, are not directly adapted to physiological use. The quantity of salts present in sea-water is a term which can be so utilized and it becomes especially valuable in view of the fact that the proportion of constituents has been shown to vary but slightly, the concentration only being subject to considerable variation. By means of the *Challenger* proportions worked out by Dittmar[2] any total salt content can be resolved into its chief constituent parts. These proportions are as follows:

	Per Cent.
$NaCl$	77.758
$MgCl_2$	10.878
$MgSO_4$	4.737
$CaSO_4$	3.600
K_2SO_4	2.465
$MgBr_2$	0.217
$CaCO_3$	0.345

It has been shown that the total salt content is directly related to the specific gravity and that one may be calculated from the other. Specific gravity determinations are made with reference to different standard temperatures. Frequently density readings are made with the temperature indicated in Fahrenheit units. These are usually referred to 60° F. as a standard temperature, and the observed density is reduced to 60° F., sp. gr. 60° F./60° F. This is easily done by means of Libbey's tables.[3] If the observed temperature is below 60° F. subtract the observed degrees of temperature from 60, multiply this difference by the correction found in the table opposite the observed temperature and *subtract* the product from the reading to be corrected. If the density is observed at a temperature above 60° F. ascertain as before the number of degrees of difference

Temp.	Correction for Reduction to 60° F.	Temp.	Correction for Reduction to 60° F.
I	II	I	II
50°	−0.000108	70°	+0.000145
51	−0.000110	71	+0.000146
52	−0.000112	72	+0.000147
53	−0.000113	73	+0.000148
54	−0.000115	74	+0.000149
55	−0.000118	75	+0.000151
56	−0.000120	76	+0.000152
57	−0.000120	77	+0.000154
58	−0.000120	78	+0.000156
59	−0.000120	79	+0.000157
60	+0.000125	80	+0.000158
61	+0.000130	81	+0.000159
62	+0.000135	82	+0.000160
63	+0.000137	83	+0.000162
64	+0.000137	84	+0.000163
65	+0.000138	85	+0.000164
66	+0.000140	86	+0.000166
67	+0.000141	87	+0.000167
68	+0.000142	88	+0.000168
69	+0.000143	89	+0.000170

[1] Published by permission of the Secretary of Agriculture.

[2] Dittmar, Challenger Reports, Physics and Chemistry, Vol. 1, Part 1, p. 138.

[3] Libbey, William, "Physical Investigations off the New England Coast," Bull. U. S. Fish Commission, 9, pp. 397–398 (for 1889).

between this temperature and 60 degrees, multiply again by the correction factor taken from the table opposite the observed temperature and *add* the resulting product to the density reading as observed on the instrument. The table of corrections is reproduced herewith.

One frequently finds density expressed as sp. gr. 15° C./15° C. This value differs so slightly from sp. gr. 60° F./60° F. that in the following discussion the treatment applied to sp. gr. 15° C./15° C. may be understood to apply to sp. gr. 60° F./60° F. without error sufficiently large to interfere with the usefulness of any biological results in which they may play a part.

More often, however, the density is expressed in terms of 15° C. referred to the temperature of maximum density, sp. gr. 15° C./4° C. Again by the help of a correction constant determined by Dr. O. H. Tittman sp. gr. 60° F./60° F. may be reduced to sp. gr. 15° C./4° C. To make this correction 0.00082 is subtracted from the value expressed as 60° F./60° F.

When the density of a sample has been reduced to 15° C./4° C. it is possible by means of Petterson's[4] determinations to ascertain the corresponding quantity of salt in the water. This tabulation gives the specific gravity readings both as sp. gr. 15° C./15° C. and as sp. gr. 15° C./4° C. of a series of sea-water samples of different density and the corresponding number of grams of total salts per liter. This latter value was determined by the silver titration method of Forchhammer.[5] Since the sea-water varies in concentration rather in the proportion of salts present, the accurate determination of any one constituent should by simple calculation give the total salts. Since Cl is present in relatively large quantity and can be determined by titration with silver nitrate to a very great degree of accuracy, it is most often used for this purpose. It has been found by Dittmar[6] that the ratio of salt to chlorine 1.8058 applies to all oceanic waters. Petterson found a slightly greater value. He carefully determined by titration the salt content of the samples just referred to and obtained a series of values which by interpolation can be used in determining either salt content from sp. gr. or sp. gr. from salt content.

Petterson's determinations are here given.

TABLE CONTAINING THE RELATIONS OF CHLORINE, SALT, AND SPECIFIC GRAVITY

Cl_2 in ‰	Salt in ‰	Sp. gr. $\frac{15°}{15°}$ Sprengel	Sp. gr. $\frac{15°}{4°}$
19.517	35.26	1.02715	1.02629
19.415	35.07	1.02701	1.02614
19.335	34.95	1.02698	1.02612
19.171	34.64	1.02668	1.02582
18.320	33.12	1.02554	1.02468
17.040	30.83	1.02377	1.02290
17.005	30.76	1.02371	1.02285
16.277	29.46	1.02261	1.02175
15.421	27.93	1.02155	1.02068
14.220	25.77	1.01983	1.01897
12.928	23.46	1.01805	1.01719
12.628	22.93	1.01761	1.01675
12.571	22.81	1.01750	1.01665
11.263	20.45	1.01570	1.01484
9.473	17.25	1.01323	1.01237
7.067	12.86	1.00987	1.00903

Since it is somewhat awkward to interpolate observed values into this series, the writer has used the data as the basis of a generalized scheme by means of which equivalents can be promptly and easily found over a range of variation of salt content somewhat greater than has been observed at Woods Hole.[7] By plotting all of Petterson's values and by prolonging the curves, the range of the table may be greatly increased though somewhat at the expense of accuracy if an extension to either great concentration or great dilution is attempted.

In the accompanying diagram (Fig. 1) the

[4] Petterson, Otto, "A Review of Swedish Hydrographic Research in the Baltic and North Seas," *Scottish Geographical Magazine*, 10, pp. 296–299, 1894.

[5] Forchhammer, G., "Om Sövandets Bestanddele og deres Fordeling i Havet," Kjöbenbavn, 1859. Engl. trans. *Philosophical Transactions*, 1865.

[6] Dittmar, Challenger Reports, Physics and Chemistry, Vol. 1, 39.

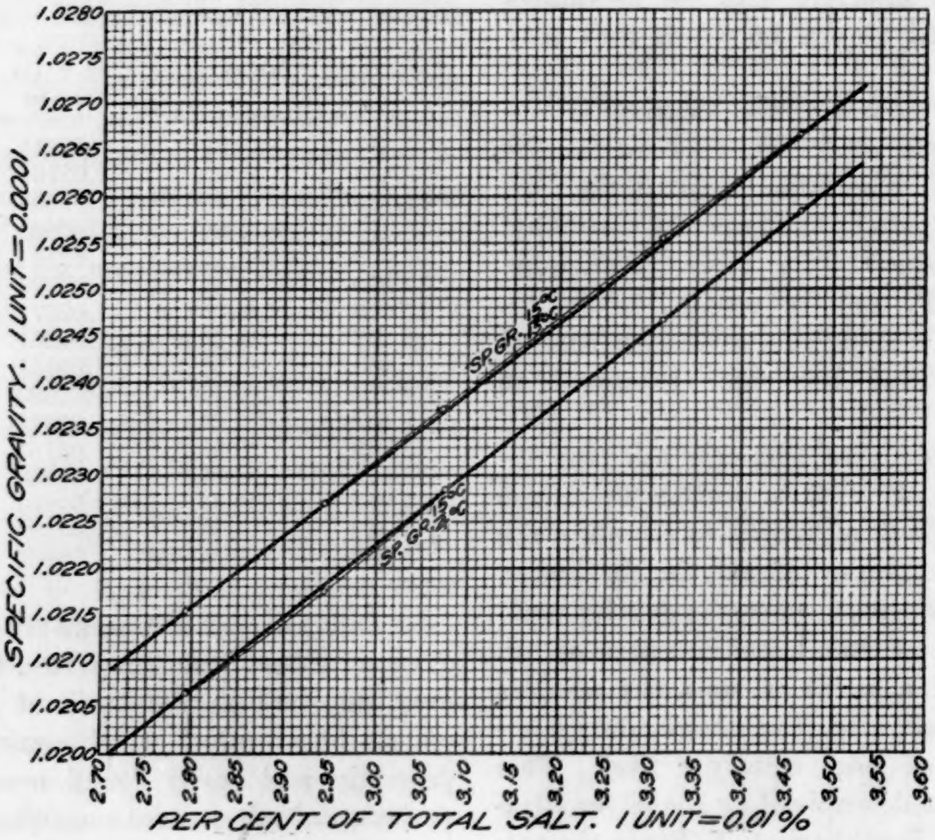
[7] Sumner, Francis B., Osburn, Raymond C. and Cole, Leon J., "A Biological Survey of the Waters of Woods Hole and Vicinity," Part I., Sec. 1, Physical and Zoological, Bull. U. S. Bureau of Fisheries, Vol. 31, Part I., 53 (for 1911), 1913.

curves expressing the ratio of sp. gr. 15° C./15° C. to total salt and of sp. gr. 15° C./4° C. to the same total salt content are drawn on a scale of absolute units. The specific gravity determination made on the basis of either of the two types of reference here mentioned, is plotted on the perpendicular axis, one unit on this axis being equal to one unit in the fourth decimal place of the sp. gr. reading. On the horizontal axis is plotted the corresponding scale of salt contents, one unit on the axis being equal to one unit in the second decimal place when salt content is stated in percentage. To use the table, the sp. gr. determined either as sp. gr. 15° C./15° C. or as 15° C./4° C. is sought on the perpendicular axis. The horizontal line on which this value stands is traced to the point of intersection with the line determined for the sp. gr. ratio adopted. This point of intersection stands directly above the point on the horizontal axis at which the total salt is indicated. This can be read with approximate accuracy by reference to the nearest given values.

To determine the sp. gr. by either system of reference of a sea-water solution containing a known quantity of salts, reverse the process just described.

This diagram does not give results having a degree of accuracy required for physical investigations, but is believed to be more accurate than will be required for use in biological work. The writer had only the convenience of biologists in mind in preparing these notes.

DIAGRAM FOR COMPUTING SPECIFIC GRAVITY AND TOTAL SALT CONTENT IN MARINE WATERS.



Any one wishing to work over a wider range and with a greater degree of accuracy may use the data of Knudsen[8] in a similar way as a basis for interpolation.

For any one who is content with even rougher approximations it may suffice to apply a fairly accurate coefficient to the density readings. Karsten[9] has pointed out that when the total salt content is divided by the number represented by the first four decimal places in

[8] Knudsen, "Hydrographische Tabellen," Copenhagen, 1901.

[9] Karsten, G., *Wissenschaftliche Meeresuntersuchungen*, N. F. 1, H. I., 170, 1896.

the sp. gr. reading a coefficient is obtained which is of use in reducing sp. gr. readings to salt content. For the range of concentration likely to be seen at Wood's Hole, *i. e.*, when sp. gr. 15° C./4° C. = 1.0210 to 1.0245 corresponding to a total salt content of 2.84 per cent. to 3.29 per cent., the salt content is obtained with a probable error less than 2 in the second decimal place by multiplying the sp. gr. reading by the factor *134.5*.

RODNEY H. TRUE
U. S. DEPARTMENT OF AGRICULTURE

ON CELL PENETRATION BY ACIDS[1]

Preliminary Note

1. THE water-soluble blue pigment[2] in the cells of a nudibranch, *Chromodoris zebra* Heilprin, is a sufficiently delicate indicator to justify its use for the study of cell penetration by acids. Water extracts of the animal, containing this pigment and other cell materials expressed by grinding, change from a deep blue color with reddish-purple fluorescence to a delicate pink hue at a hydrogen ion concentration of $p_H = 5.6$[3]; the acidity of the body fluids of *Chromodoris* averages $p_H = 7.4$ (27°). The indicator promptly flocculates, in the form of a greenish-blue precipitate, leaving a blue solution, at $p_H = 7.6$. Within the epidermal cells the pigment is also turned green, so that it may be used to measure the penetration of alkalies; it gives results concordant with those obtained with a great variety of tissues by the neutral red method (Harvey[4]), and with neutral red-stained *Chromodoris* cells lacking the blue pigment.

The pigment occurs in two forms: as granules scattered through the superficial and deeper tissues, and dissolved in clear globular bodies located within the cells of the outer epithelium, especially along the edges of the mantle and foot. It is totally insoluble in anhydrous acetone, ether, chloroform, xylol and oils. The globules containing it do not stain with fat dyes. I conclude that the pigment is held naturally in water solution.

2. Direct measurements of the speed with which acids penetrate protoplasm were first given by Harvey,[5] who determined the time required for the testis of *Stichopus ananus* to change in color when immersed in 0.01 *N* solutions of a number of acids. I have used pieces of the lateral mantle edge of *Chromodoris* in a similar way, precautions being taken to insure comparative uniformity of the pieces in the different tests, and find that *at this concentration* (0.01 *N*) the acids employed when arranged in the order of increasing penetration-time form the series shown in Table I. Comparison of this list with

TABLE I

Penetration of Acids from 0.01 N Solutions

No.	Acid.	Time, Minutes.	
		Chromodoris. Mantle Edge. 27°.0.	*Stichopus ananus, testis* (Harvey).[6]
1	Valeric (Iso-)........	1.9	2–4
2	Salicylic........	3.5	0.25
3	Formic........	4.5	2–4
4	Hydrochloric........	7.6	} 9–11
5	Nitric[7]........	8.4	
6	Sulphuric........	8 5	
7	Lactic........	8.6	
8	Oxalic........	8.8	12–15
9	Tartaric........	13.5	30
10	Citric........	16.0	40
11	Butyric........	19.0	} 45–60
12	Acetic........	75.0	

[1] Contributions from the Bermuda Biological Station for Research, No. 39.

[2] Crozier, W. J., 1914, *Journal of Physiology*, Vol. 47, p. 491.

[3] This point changes somewhat with the age of the extract, in the case of alcohol (95 per cent.) and other permanent solutions of the pigment. The p_H values given were obtained by titration with phosphate and acetate mixtures, checked by gas chain measurements on alcohol and formalin solutions of the pigment.

[4] Harvey, E. N., 1914, Papers from the Tortugas Lab., Vol. VI., p. 133.

[5] Harvey, E. N., 1914, SCIENCE, N. S., Vol. XXXIX., p. 947.

[6] Only those acids which I have studied have been taken from Harvey's table, which includes a number of others.

[7] The differences in penetration-time for Nos. 5–8 are slight at this concentration, but their separation is justified on the basis of the dilution curves.

that of Harvey discloses that the relative penetrating power of the acids at this concentration is practically identical in the two cases. Some of the differences may be due to the temperatures at which the two sets of experiments were made. The figures for the *Chromodoris* tissue represent the mean of ten concordant experiments at a uniform temperature of 27°.0.

3. Examination of the penetration time of these acids over a *range of concentrations* (0.1 *N* to 0.001 *N*) shows, however, that the series established at the single concentration (0.01 *N*) gives an entirely misleading picture of the penetrating powers of the different acids, which is better judged by the nature of the penetration curves as a whole. According to this view the order arrived at is seen in Table II. The acids studied may be arranged

TABLE II

Penetration Power of Acids

Acid	Ionization Constant (*K*)
Group I:	
Hydrochloric	(100)
Sulphuric	(100)
Oxalic	3.8
Nitric	(100)
Group II:	
Formic	0.0214
Salicylic	0.102
Valeric (iso-)	0.0017
Lactic	0.0138
Tartaric	0.1000
Citric	0.0870
Butyric	0.00149
Acetic	0.00180

in two groups on the basis of the character of their penetration-dilution curves.[8] The curves of the second group are all more or less parallel and uniformly concave toward the axis of penetration-time, whereas the curves of acids of the first group (up to 0.002 *N*) are from the beginning concave toward the axis of dilution. The curves of the two sets cut across one another, as do also some of the curves within each set. The acids of the first set give visible evidence of penetration at higher dilutions ($n/750$) than do those of the second group.

The separation of these two groups of acids is further warranted by the fact that, within certain time-limits, a preliminary exposure of the *Chromodoris* tissue to the action of acids of the second group does not hasten the penetration of acids of the first series, but does that of other acids of the second set.

4. The acids included in my group I. of Table II. are all acids of strong ionization, while those of the other group are of low acid strength. To this extent the rôle of ionization in determining permeability toward acids is made clearer than has hitherto been the case, and it seems probable that these two kinds of penetration curves represent at least two different and distinct methods whereby acids may gain access to the interior of cells. Within each of the two sets of acids the degree of ionization is less important in controlling the speed of penetration. Formic acid occupies a somewhat peculiar position, as do also butyric and valeric; the first substance shows a dilution curve more nearly approaching that of the strong acids, agreeing with its constitution, but the relative positions of butyric and valeric in the series are more likely to be accounted for by their rather high solubility in lipoids.

All the evidence so far available indicates that acids penetrate and combine to various degrees with one or more of several constituents of the cell surface. It is certain, at any rate, that the "lipoid theory" of permeability is not even approximately complete as an explanation. Further attempts to elucidate the significance of the apparently quite general uniformity in the order of cell penetrability for various acids in different animals must await the study of a larger series of substances, especially with reference to the action of acids on penetrability for other acids.[9]

W. J. Crozier

Agar's Island, Bermuda

[8] This series has an important bearing on the interpretation of sensory stimulation by acids, a matter which first turned my attention to this problem.

[9] Details, covering additional points not here considered, will be found in a paper to appear in the *Journal of Biological Chemistry*.